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THE TRANSACTIONS

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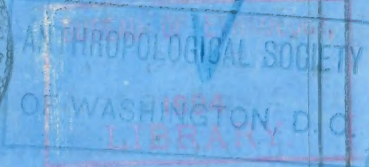
THE VICTORIA INSTITUTE,

OR

Philosophical Society of Great Britain.

THE SECOND QUARTERLY PART OF

VOL. XXVII.



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JOURNAL OF  
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EDITED BY THE HONORARY SECRETARY,  
CAPTAIN F. W. H. PETRIE, F.G.S., &c.

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\* \* \* *The Institute's object being to investigate, it must not be held to endorse the various views expressed at its meetings.*

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## ORDINARY MEETING.\*

REV. PREBENDARY R. THORNTON, D.D., V.P., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Election took place :—

MEMBER :—Peter F. Wood, Esq., Kent.

The following paper was then read by the author :—

*HOW THE WATERS OF THE OCEAN BECAME SALT.*

By Prof. EDWARD HULL, LL.D., F.R.S., F.G.S.

**T**HERE are many things in the world around us to which we are so accustomed from childhood that we never stop to enquire why they should be so. That rivers and lakes should consist of fresh water, and that the sea should be formed of salt water, seems so natural that we consider them as not only matters of course, but essential to the physical economy of the world; and if perchance our attention is called to the fact that some inland lakes are formed of salt water we proceed to investigate the cause of so unusual an occurrence—one which being exceptional requires special explanation. But how few of the thousands and millions who traverse the ocean or dwell upon its shores put to themselves the question “Why are its waters salt?” And this, notwithstanding that it is daily receiving supplies of fresh water both from the rain which falls upon its surface and from the rivers which empty themselves into it. Clearly there is something here which *does* require special investigation, a question which *does* need solution, because, as far as the sup-

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\* 3rd of 28th Session.

plies afforded by the rain and rivers are concerned, the ocean waters ought apparently to be fresh rather than salt.

<sup>2</sup> In using the terms "fresh" and "salt" here, and in the following pages, I do so in the popular sense of the words. Scarcely any natural water, except rain, is absolutely free from dissolved salts. All rivers contain them to a greater or less extent, as do also the waters derived from wells and springs. Such waters, however, are called (and properly called) "fresh," which does not necessarily mean water absolutely devoid of salts in solution; but when the proportion of salts is so great as to cause the water in which they are dissolved to appear "salt" to the taste, then the term salt water or brine is applied to them. The varieties of saline waters and the degrees of salinity are innumerable, and their properties and uses vary accordingly. There are the salt waters of the Dead Sea—so acrid as to be nauseous to the taste; there are those of the ocean, not so acrid: there are the varieties of mineral waters, and the brine springs highly impregnated with sodium chloride. But it is not necessary to go further into this branch of the subject; all that is necessary is to understand clearly the meaning of the terms we employ, and in the following essay I shall use the words fresh, brackish, and salt as applied to water in the sense they are popularly understood.

<sup>3</sup> But before entering upon the discussion regarding the cause or origin of the saltiness of the oceanic waters we may endeavour to ascertain whether this highly saline character has characterised these waters throughout a very long period of geological time. Now the evidence we can safely rely upon in this part of our enquiry is mainly derived from the character and affinities of the organic forms of past geological ages. At the present day the molluscan and other forms which inhabit the ocean waters are distinguishable from those which inhabit fresh water lakes and rivers, while there are numerous others, such as the *Actinozoa* or corals, star-fishes, crinoids, sea-urchins, and echinoderms, exclusively confined to oceanic waters at the present day. Amongst the molluscs the Brachiopods (molluscoidea) and the Cephalopods are specially characteristic of oceanic waters of the present day, and are therefore of special value in the attempt to determine the character of the waters which they inhabited in past geological times.

<sup>4</sup> Now representatives of all these forms are found not only in Tertiary, Secondary, but even in early Primary or

Palæozoic strata. Not only in the Cretaceous and the Jurassic strata, but also in the Carboniferous, Devonian, and Lower Silurian (Ordovician) formations do we find corals, crinoids, starfishes, sea-urchins, various forms of Brachiopods and Cephalopods, differing indeed specifically from, but sometimes generically related to, those of the present day. The forms which are thus preserved to us in a fossil state are only those which were furnished with a stony or horny skeleton or integument. Many other forms there were which had no calcareous skeleton, and consequently have not been preserved in a fossil state, but which are represented in the ocean waters of the present day; and if these be allowed for, it becomes clear that amongst the invertebrate forms of marine life, those of the present day were largely represented in very early geological periods.

<sup>5</sup> Such being the case we are justified in coming to the conclusion that the waters of the ocean must have been salt from very early geological times; but it by no means follows that they were fully as saline as those of the present day.

The forms of life which require the high salinity of existing ocean waters were possibly represented by others capable of sustaining life when the salinity was only half as great as it is now. We know that some forms, such as those of the oyster, cockle, &c., are capable of surviving in the Baltic, or of ascending estuaries, where the water is almost brackish. Degrees of temperature, purity (or freedom from sediment), and other conditions were probably of greater importance in determining the existence of life than degrees of salinity. Adaptability to the conditions of environment has doubtless been a law of nature amongst marine forms as well as those of the air and the land throughout all past time.

<sup>6</sup> It is scarcely necessary to state here that the occurrence of beds of rock salt in several formations, especially in the Trias of the British Isles and of Europe, affords no evidence as regards the degree of salinity of the sea water in geological times. At no period have the waters of the ocean been so saturated with saline matters as to admit of the deposition of beds of rock salt. It has sometimes been suggested that such deposits may have been formed by the accidental accumulation of sand bars, owing to which portions of the ocean have been cut off from the main mass and the salts have been deposited as the waters have decreased and become supersaturated by evaporation. But the mode of

occurrence of the known beds of rock salt lend little support to this view; and recent investigations have led to the conclusion that deposits of rock salt have been accumulated over the floors of inland salt lakes like that of the Dead Sea in Palestine, along whose banks such deposits occur in the form of terraces which once formed the bed of the inland lake itself, when at a higher level than at present, but owing to the lowering of its waters are now exposed along its western margin, as in the case of the terraced hill known as Jebel Usdûm. Another fatal objection to the view of the marine origin of salt rock is to be found in the fact that this rock generally consists of nearly pure chloride of sodium, while ocean water contains large proportions of the chlorides of calcium, magnesium, and potassium, the precipitation of which would result in a deposit very different from that of the rock salt of Cheshire and Worcestershire, which is composed of 98·30 per cent. of chloride of sodium and only small traces of other salts.

7 But in addition to the evidence derived from organic forms of the primæval ocean we apparently possess very remarkable direct evidence that the waters were highly saline. It is known that some strata of the Upper Silurian period in North America are saliferous, constituting the Onondaga salt group and the Trenton and Chazy limestone series.\* These strata are characterised by large numbers of marine organisms, and there can be no doubt that they were formed in the waters of the Silurian seas. They also yield large quantities of saline waters which are used in commerce, and in which chloride of sodium predominates; and as the strata are often in the condition of basins below the level of the outer ocean, Dr. Sterry Hunt has inferred that the waters with which they are saturated were originally those of the Palæozoic ocean in which the strata were deposited. In other cases, however, where the strata are upraised above the ocean level and highly inclined, the same author considers that surface waters have gradually replaced those originally contained in the strata.† Thus we are justified in inferring, not only from organic, but from direct physical evidence, that the waters of the early Silurian oceans were salt.

8 On examining samples of water taken from the open

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\* Dana states that in the State of New York the salt is made from strong brine by sinking wells varying from 150 to 340 feet in depth. It takes from 35 to 45 gallons of this water to make a bushel of salt, whereas it takes 350 gallons of sea water for the same result.

† *Chemical and Geological Essays*, p. 104.



ocean of various regions and far from land, it has been found that the proportions of salts and carbonates do not much vary. This is doubtless owing to that wonderful system of currents by which the waters are kept in a state of perpetual movement, and owing to which there is a constant interchange of the warmer waters of the equatorial regions with the colder of the polar. Sea water is essentially a chlorinated alkaline mineral water, the saline contents of which consist chiefly of sodic, magnesian, potassic, and calcic chlorides and sulphates; together with a number of other substances in much smaller proportion. The total amount of dissolved contents in the water of the open ocean varies from about 28 to 39 grammes per litre. Forchhammer fixes the mean amount of such contents at 34·404 grammes per litre,\* and the mean proportions of the constituent substances to each other and 100 parts of chlorine are as follows†:—

Sodium.	Magnesium.	Calcium.	Chlorine.	Total saline constituents.
14·26	6·642	3·114	100	181·10

9 What is most striking in this analysis is the large proportion of chlorine, and the greatest difficulty we are met with in order to account for the salts of sea water is the abundance of this gas. Recollecting that chloride of sodium is the most abundant salt both in most salt lakes and in sea water, we are justified in seeking for a solution to our problem by an examination into the mode of origin of salt lakes.

10 Now there is one peculiarity which characterises all salt lakes over the surface of the globe, namely—that they have no outlet; they are closed lakes. Whether we take the case of the salt lakes of Western America, those of Central Asia and the Dead Sea, we shall find that they are not drained by rivers.

11 In such cases the lakes are constantly receiving supplies of water from streams and springs, but do not give it off in the same manner, inasmuch as it is evaporated into the air as fast

\* *Phil. Trans.*, civ, 303 *et seq.*

† *Watt's Dict. Chem.*, Vol. v, 1019.

as it falls. In the case of fresh water lakes it is otherwise. Here the water of the streams which enter the lake is at least partially discharged by means of rivers flowing out, in consequence of which the water remains fresh, as the saline ingredients are carried away as fast as they are delivered. Of these two varieties of inland lakes we have remarkable examples in the case of the Dead Sea and the Sea of Galilee. In the former case the river Jordan entering at the northern end keeps up a constant supply, but this lake, which is about 1,292 feet below the level of the Mediterranean, has no outlet, in consequence of which the water supplied by the Jordan passes away into the atmosphere in the state of vapour. In the case of the Sea of Galilee it is otherwise. The river which enters at the north passes out again at the south; hence the water of the lake is fresh and supports an abundant fauna of fishes and molluscs, while the waters of the Dead Sea are (as the name indicates) absolutely destitute of living beings, and fish entering it from the Jordan immediately perish. If there had been an outlet to the southwards from the Dead Sea into the Gulf of Akabah, and a continuous stream had been flowing from the time the depression was formed, the waters of the Dead Sea would have only differed from those of the Sea of Galilee by a somewhat greater proportion of salts and carbonates. Several other examples might be cited, but those of the Sea of Galilee and the Dead Sea are the most familiar and striking.

<sup>12</sup> There are two ways by which we may account for the salinity of the ocean waters from very early periods of geological time. First, by supposing that the primæval waters were saturated with acid gases which were held in suspension in the vapour surrounding the incandescent globe; or secondly, that the salinity resulted from a process resembling that by which salt lakes of the present day have been formed.\*

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\* Of the former method Dr. Sterry Hunt may be considered the chief exponent, and in order that I may not unintentionally misrepresent his views I will give them here in nearly his own words. Referring to that period in the physical history of our globe in which it may be presumed to have been in a molten state surrounded by an atmosphere and an envelope of vapour of water, he says:—"There would be the conversion of all the carbonates, chlorides and sulphates into silicates, and the separation of the carbon, chlorine and sulphur in the form of acid gases which, with nitrogen, vapour of water, and a probable excess of oxygen could form the dense primæval atmosphere. The resulting fused mass would contain all the bases as silicates, and must have resembled (when consoli-

13 We must, I think, concur with Dr. Hunt that from some cause or other, chlorine largely abounded in the waters of the primæval ocean, as by far the greater proportion of the salts are chlorides, and chlorine is but very slightly represented in river waters at the present day.

14 In contrast to the above, which may be called "the chemical theory," we may now consider that which may be called "the geological theory," though it very much depends upon certain chemical processes.

15 If we compare the analyses of waters brought down by rivers into the ocean at various parts of the globe we shall find that the matters in solution are very much the same as those which we find dissolved in oceanic waters; the proportions are doubtless immensely different, but the ingredients are essentially similar. Now, what are the dissolved ingredients of river waters? They are calcium, magnesium, sodium, potassium, a little iron, silica, alumina, and other matters, in combination with carbon-dioxide (carbonic acid gas), sulphuric acid, hydrochloric acid. Of these the carbonates of lime and magnesia are the most abundant, but chlorides of sodium and magnesium are almost always present even in waters where there can be no suspicion that they have been introduced by any artificial means. These constituents are also found in even larger proportions in the waters of natural springs; and in such chlorine occurs, sometimes in considerable quantity, in combination with sodium, magnesium, and potassium. Spring water coming as it does directly from the strata, or from rocks of various kinds, is

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dated ?) certain furnace slags or volcanic glasses. The atmosphere charged with acid gases which surrounded this primitive rock must have been of great density. Under the pressure of a high barometric column condensation could take place at a temperature much above the present boiling point of water, and the depressed portions of the half-cooled crust would be flooded with a highly heated solution of hydrochloric and sulphuric acids, whose action in decomposing the silicates can easily be understood. The formation of the chlorides and sulphates of the various bases and the separation of silica would go on until the affinities of the acids were satisfied, and there would be a separation of silica taking the form of quartz, and the production of sea-water holding in solution, besides the chlorides and the sulphates of sodium, calcium, and magnesium, salts of aluminium and other metallic bases. The atmosphere being thus deprived of its volatile chlorine and sulphur compounds, would gradually approximate to that of our own time, but would differ in the greater amount of carbonic acid gas." *Chemical and Geological Essays*, p. 40 (1875).

generally free from any external or artificial ingredients, hence it may be regarded as the chief source of supply of the carbonates and salts found in streams and rivers. If we enquire what is the origin of spring water, the reply is simple. It is water which originally falling on the surface as rain or snow has percolated downwards into the rocky crust, and taking up the soluble matters with which it comes in contact, bursts forth at the surface along lines of fault, fissure, or other natural ducts. The relative proportions of the ingredients of sea water and of rivers or lakes may be gathered from the following selected examples:—

PROPORTION OF SOLUBLE INGREDIENTS IN THE WATERS OF THE OCEAN AND SPECIAL LAKES AND RIVERS.  
Parts in 1,000,000.

	Total Solid Contents.	Ca.	Mg.	Na.	K.	CO <sub>2</sub> .	SO <sub>4</sub> .	Cl.	Br.	SiO <sub>2</sub> .	Fe.	Authority.
Atlantic Ocean (41° 18' N., 36° 28' W.) ..	38,400	556	1,198	11,719	668	..	3,029	20,839	387	..	..	Von Bibra.
Caspian Sea (2 versts S.W. of Pischmoi) ..	6,296	191	409	1,444	139	77	1,337	2,737	..	..	40	Göbel.
Dead Sea.. ..	240,483	9,000	19,883	47,918	6,385	..	497	154,442	2,176	..	11	Herapath.
Rhine at Basle .. ..	169	55	4·8	0·6	..	86	15·4	1·5	..	2·1	..	Pagenstecher.
Aar, near Berne .. ..	216	66	10·0	0·3	..	103·3	33·7	0·3	..	2·7	..	"
Severn, Wales .. ..	38·7	3·0	2·0	6·1	1·2	2·0	12·8	8·2	..	2·0	..	Frankland and Odling.
Thames at Twickenham	321	83·8	4·7	9·2	4·2	119·9	31·4	14·2	..	3·9	..	Clark.
Thirmere .. ..	51·5	4·3	1·2	4·9	..	10·9	7·5	11·0	..	0·7	..	Way.
Bala Lake .. ..	27·9	1·5	0·8	3·9	0·9	1·9	3·5	7·3	..	0·3	..	Frankland.

Decimals are omitted in the case of sea waters.

16 From the above results of the analysis of various waters, it will be seen that there is no essential difference between the waters of the ocean and those of lakes and rivers except in the proportions of the dissolved ingredients. There are, of course, occasionally substances specially abundant, as is the case with bromine in the waters of the Dead Sea, probably derived from the volcanic district on its borders; on the other hand, silica ( $\text{Si O}_2$ ), which is not mentioned in the waters of the Atlantic Ocean in the analysis of Von Bibra, is certainly present in those waters, and supplies the material from which sponges, diatoms, and radiolaria build up their skeletons. It will be observed also that chlorine and sulphuric acid is present in all the waters, and these gases uniting with the alkalis, give rise to the salts which are so abundant in the waters of the ocean and of closed lakes.

17 In considering the manner in which springs and surface waters have become impregnated with salts and carbonates, we have to recollect that all rocks decompose in presence of the atmosphere. This is mainly due to the carbonic acid (carbon-dioxide) contained in the air and rain water, which acts upon felspathic rocks, composed, as we have seen, of double silicates of alumina, potash, and soda. Ebelman has well explained the process by which basaltic and similar rocks are decomposed under the influence of the atmosphere. The carbonic acid (carbon-dioxide) combines with the lime and magnesia, while the silica is liberated in a soluble form. The felspar is more stable than the pyroxene and hornblende, but it ultimately gives way, forming a hydrous silicate of alumina. Thus we can account for the presence of carbonates of lime and magnesia, free silica, and by a further process in presence of sulphuric acid and chlorine of the various sulphates and chlorides.

18 Now, as bearing on the fact of sodium chloride (or common salt) being the chief ingredient in oceanic waters, as well as in those of the Dead Sea and most salt lakes, we must recollect that the soda-felspars are much more soluble than the potash-felspars, and on this account we have probably a true cause of the predominance of sodium chloride. The rocks composed in the main of such felspars as labradorite, albite, oligoclase, and andesine, were therefore more powerfully acted upon than those composed of orthoclase and sanidine; but even in these cases many orthoclase granites contain proportionate quantities of the soda felspars

such as oligoclase and albite, and the decomposition of these components would hasten that of the less soluble varieties.

<sup>19</sup> It seems not improbable from certain considerations connected with the organic structures of the ancient world, that carbon-dioxide was more abundant in the atmosphere of Palæozoic times than at present. The enormous quantity of carbon which must have been extracted from the air during the Carboniferous period in order to the formation of the beds of coal at intervals all over the world, seems to favour this view; and if this be so, then we may suppose that previous to the Carboniferous period, the air was highly charged with carbon-dioxide, and the process of decomposition on the land surface was carried on with even greater rapidity than at the present day; but even had this not been the case, it only requires a sufficiently long period in order to bring about the chemical reactions necessary to the salinification of the oceanic waters.

<sup>20</sup> We are now approaching the conclusion of our enquiry. From the examples of closed lakes we can determine the process of salinification with the utmost certainty. Throughout greater or shorter periods, these lakes have been receiving the waters of rivers bringing down, both mechanically suspended sediments and chemically dissolved salts, silicates and carbonates. The sediments are precipitated over the bottom of the lakes, and the water being carried off into the atmosphere in the form of vapour as fast as it enters, leaves behind the dissolved ingredients. These necessarily augment in quantity, and ultimately the waters of the lakes become saturated with salts and carbonates, which are then deposited.

<sup>21</sup> Now the ocean is a closed lake of enormous magnitude. Throughout all geological time it has been receiving continual supplies from rivers bringing down not only sediment, but salts and carbonates, together with free silica, in solution. The sediment is deposited over the ocean floor, and generally not far from the lands, while the dissolved ingredients are carried by the currents into all parts. Meanwhile the ocean surface is constantly giving off, particularly over the equatorial regions, enormous quantities of vapour which are carried into the higher regions of the atmosphere, and are precipitated in the form of rain and snow over the lands. Part of course falls on the sea again, but the greater quantity falls on the land surfaces, and is returned to the

ocean in streams charged with a fresh supply of the salts and carbonates it had left behind in the ocean.\* The consequence of this process must clearly be that the saline ingredients have been increasing in the oceanic waters from the earliest periods down to the present day. As regards the carbonates of lime and magnesia, and the silica which are being carried into the ocean by the rivers, we have no difficulty in accounting for their uses. Of these materials, the shells and skeletons of the molluscs, echinoderms, reef-building corals, foraminifera, sponges, radiolarians and diatoms and other forms are built up, and as these structures are continually being formed, and the materials solidified as fast as they enter the oceans, there is no reason why they should augment. Hence the proportion of carbonates of lime and magnesia in the ocean waters may be very much the same now as it was in Silurian and Carboniferous times.

<sup>22</sup> We are thus brought to the conclusion that the saltiness of the sea may have originated in very much the same way as has that of the Dead Sea, Lake Oroomiah, or the Great Salt Lake of Utah, or many others which might be named, and which possess in common the characteristic of having no outlet. When the great envelope of vapour which surrounded the incandescent globe began to condense upon its cooling surface, the resulting waters, though containing, as Dr. Sterry Hunt supposes, acid gases, were destitute of saline ingredients. The process of salinification began with the first streams which entered the seas from the bordering uplands, and this process carried on throughout the long ages preceding the Silurian period brought the waters to a condition suited to sustain the life of forms of inhabitants representative of those which inhabit the ocean at the present day. These long ages may be supposed to include, not only the Archaean and Azoic periods, but that during which the first crust was in course of formation over the incandescent globe.

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\* This process of evaporation and supply by rivers is accurately described in the Book of Ecclesiastes i, 7.



The CHAIRMAN (the Venerable Archdeacon ROBINSON THORNTON, D.D., V.P.)—I am sure all have listened to this paper with much interest and desire to return the author their hearty thanks.

Captain F. PETRIE (Hon. Sec.)—Some communications have been sent by those unable to be present to-day. The first is from Professor JOHN TYNDALL, F.R.S. :—

“ Hind Head House, Haslemere.

“ DEAR SIR,

“ I have read with interest the paper by Professor Hull which you have been kind enough to send to me.

“ The theory which he enunciates is set forth with lucidity and scientific truth.

“ Yours very faithfully,

“ JOHN TYNDALL.

“ To Captain F. Petrie.”

The next is from Professor JOSEPH PRESTWICH, D.C.L., F.R.S. :—

“ There is one point in the interesting address of Professor Hull in which I cannot agree. Speaking of the great deposits of salt in the Triassic and other strata, he says: ‘ Another fatal objection to the view of the marine origin of rock-salt is to be found in the fact that this rock generally consists of nearly pure chloride of sodium, while ocean water contains large proportions of the chlorides of calcium, magnesium, and potassium, the precipitation of which would result in a deposit very different from that of the rock-salt of Cheshire and Worcestershire, which is composed of 98·30 per cent. of chloride of sodium and only small traces of other salts.’ As I have explained elsewhere (*Geology*, vol. ii, p. 160) it seems to me on the contrary probable that these salt beds were formed by the deposition on the evaporation of sea water in lagoons or inland lakes, the cause of the difference of composition being the different solubility of the various salts existing in sea water. Thus the sulphate of lime, which is the most insoluble, is deposited first, and this substance is always found associated with rock-salt, while the more soluble salts of potash and magnesia, which require a greater degree of concentration than the chloride of sodium (rock-salt), resist deposition till the last. Thus in the salterns on the coasts of the Channel the sea water let in first deposits the more insoluble sulphate, and then, when removed to other pans, deposits its chloride of sodium with but traces of the other ingredients, whilst in the mother

liquor which remains are to be found the more soluble bromides and chlorides of potassium, &c. The result is that the salt obtained in this manner direct from sea water corresponds almost exactly in composition with the rock-salt of Cheshire, as the following table will show:—

	Salt from salterns.	Rock-salt, Cheshire.
Chloride of sodium	98·80	98·32
Other chlorides	0·50	0·39
Bromides	none	none
Sulphates	0·70	0·62
Other ingredients	none	0·67
	100·00	100·00

“Thus, while sea water contains about 78 parts in 100 of chloride of sodium or common salt, the salt procured from it by evaporation consists of 98·80 parts in 100, which corresponds within a fraction with the proportion (98·32) existing in rock-salt.”

The third is from Mr. J. POSTLETHWAITE, F.G.S.:—

“I have read Professor Hull’s paper on ‘How the Waters of the Ocean became salt,’ with much interest, chiefly because my attention had been directed to the subject whilst endeavouring to investigate the source of certain mineral springs, near Keswick, for the purpose of laying the results of such investigation before the members of the Cumberland and Westmorland Association for the Advancement of Literature and Science, at their Annual Meeting in 1886. (See *Trans. C. and W. Assoc.*, vol. xi, p. 142.)

“The existence of those springs is a further confirmation of Professor Hull’s statement that ‘from direct physical evidence the waters of the early Silurian oceans were salt’ (sec. 7). The salt spring at Brandley Mine, on the margin of Derwentwater, issues from the Skiddaw Slate, in the lower part of the Ordovician (Lower Silurian) System; it contains a large amount of mineral matter in solution, namely, 203·78 grains per imperial pint, consisting of:—

Chloride of calcium	...	...	87·67 grains per pint.
“ „ magnesium	...	...	1·53 „ „
“ „ sodium	...	...	110·23 „ „
Sulphate of magnesia	...	...	4·35 „ „

Total 203·78

“The large amount of chloride of sodium proves that a large bed of this rock salt exists beneath Catbells and Maiden Moor, and this bed must have been formed in the early part of the Ordovician Age. The quantity of water issuing from this spring is, on the most moderate computation, about 150 gallons per minute, and the quantity of salt (chloride of sodium) contained in it amounts to about  $18\frac{3}{4}$  lbs., which gives a total of 12 tons 3 cwt. every twenty-four hours, 4·434 tons every year, or 443·400 tons in 100 years, and this has been going on for ages. Moreover, there is another spring at Saltwell Park, about three-quarters of a mile south of Brandley Mine, precisely the same in quality, but the quantity of water issuing from this spring is probably not more than one-third of that issuing from Brandley Mine. (When the mine was being worked it was found sometimes necessary in dry seasons to use the salt water, for a short time, for supplying the engine boiler, and I have seen cartloads of salt taken out of the boiler on these occasions.) The quantity of salt which is being continually conveyed away by these springs shows that the bed from which it is obtained must be very extensive.

“The presence of chlorine in these springs is also a confirmation of the opinion quoted from Dr. Hunt’s essay, to the effect that ‘chlorine largely abounded in the waters of the primeval ocean.’”

Mr. DAVID HOWARD, F.C.S.—I am sure all those who have paid attention to this subject must feel a great debt of gratitude to the author of the paper for the very careful research he has given to it. Of course it is not an easy subject, it is rather readily assumed either that the beds of rock salt are the source of the saltiness of the ocean or *vice versâ*, and yet what seems so simple a thing is not really easy to understand. To begin with it is a puzzle for chemists to make from sea water salt of the marvellous purity of some beds of rock salt which are almost absolutely pure, and it is a remarkable fact that they are free from those elements which adhere most persistently to salt made from sea water. On the other hand, if one studies formations which undoubtedly are the result of the drying up of portions of sea or inland lakes, one is struck by the excessively mixed character of the resulting beds—as for instance in the nitre beds of South America, which are evidently the result of drying up—the different strata are formed of different substances which follow one another as you would expect from their solubilities, and very remarkable substances some of them seem. They are very difficult to make artificially,

and yet it is clearly seen how they are formed. It is very easy to find that some of the very substances obtained from these beds are obtained from sea water. Undoubtedly there are very minute traces in all sea water of boron components, which is found crystallised in the form of boro natro calcite in these nitre beds. You must evaporate an enormous quantity of sea water to get evidence of these substances. Boron you can find with the aid of the spectroscope, but iodine, which also abounds in these beds, is extremely difficult to obtain from salt water; but the processes of certain sea weeds, of obtaining it, for the physical requirements of the plant come to our aid, and it was first discovered from the ashes of these sea weeds, which contain a considerable quantity. I believe the more you study the chemistry of sea weeds the more confidence you will have in the value of this admirable paper.

REV. F. A. WALKER, D.D., F.L.S.—There are two points upon which I should be glad to be permitted to ask for information. First:—Is it not a fact that certain seas do very greatly differ from one another in amount of salinity? the waters round the Channel Islands are said to be very salt. Secondly:—Is it the case that waters at a great depth down, say one mile or more, differ in their respective degree of salinity from the surface waters? would their smaller proportion of salinity tend to account for the total absence or great scarcity of organic life at a considerable depth, or is this absence or scarcity solely attributable to the absence of light at a great depth?

THE CHAIRMAN.—There is a theory that the globe was once incandescent and probably surrounded by a quantity of acid vapour called carbon-dioxide, and chlorine vapour especially; if that theory be true does not Professor Hull think that the chlorine vapour and the sodium vapour would have been present and have united themselves, and to the union of these two vapours the immense supply which we have of chloride vapour may be due.

THE AUTHOR.—Although the speakers are few they have given me enough to do if I am to answer their questions. But I am rather disappointed that I have had no what I may call downright opponent to meet.

THE CHAIRMAN.—I think you cannot expect anybody to criticise so convincing a paper.

THE AUTHOR.—Then I will, with the best grace I can, give my

replies. With regard to Mr. David Howard's remarks, he referred to the nitre beds of Western America. I have not seen those, they are very exceptional and very valuable; but I think he will, perhaps, find they are exceptional in this respect, viz., that they are all situated in volcanic regions, and nitre, as we know, is a volcanic product, and these are probably laid down in inland lake beds which have derived their supply of that salt from volcanic sources. It is quite true what he said about the possible existence of substances in such small quantities that in an ordinary—or indeed more than ordinary—chemical analysis they might be passed over; and I have referred in my paper to the case of free silica in solution in the waters of the ocean, which we know must be there, because from that the silicious sponges, diatoms, and other forms have built up their skeletons. Dr. Walker has given me some questions to answer, but I am afraid if I entered into them exhaustively I should keep you here too long. I will endeavour, therefore, to reply as shortly as possible. I think the first question is whether the salinity of the different parts of the ocean varies? No doubt it does to a certain extent. The surface portions of the ocean are not so saline as those of deep water; and, of course, waters which are in proximity to the outlets of large rivers are necessarily less saline than those which are in mid-ocean; but as regards the absence or prevalence of marine life in the ocean it is probably much more due to the difference of temperature than to salinity. Now the "Challenger" and other expeditions in their soundings over almost all parts of the ocean, adduced this remarkable and interesting fact—that the very deep parts of the ocean, even under the equator and the tropics, are intensely cold—almost as cold as the Arctic waters;—differing very slightly from the freezing point of water, viz., 32° F. In consequence of that, temperature is, as we know, the main factor in determining the presence or absence of animal forms in the ocean. There are many forms which will survive and flourish in warm water and will not do so in temperate water—much less will they do so in cold water; and after going down, say 3,000 fathoms, the temperature of the ocean does not vary much in any part of its floor area from the freezing point of water. Dr. Carpenter, we know, founded on that the very beautiful theory with regard to oceanic circulation which seems to be borne out by this fact. He showed that not only is there a surface circulation—the currents with which we are familiar, which

circulate over the surface of the ocean—but a vertical circulation. As the waters of the Arctic and Antarctic regions pass down—at a very slow rate it is true, but still they do pass down—along the bed of the ocean towards the equator; those on either hand gradually rise and replace the warm water which is constantly given off by the surface currents in the equatorial regions. As to the theory which the Chairman has mentioned that sodium may have been in a gaseous condition in the original highly heated circumincumbent air surrounding the incandescent globe—sodium gives a very marked line indeed in the spectroscopic analysis of the sun and of many of the heavenly bodies. But my paper refers to a more advanced stage in its course of consolidation, and I start from the period in which sodium and calcium would have entered into combination in the formation of rocks during the cooling of the crust owing to the radiation of the original heat into space. Crystalline rocks of the granitic or volcanic type may have been thus formed, and then the sodium would of course be in a combined state. It is the reaction of the supposed highly saline waters, which would still have remained as an envelope outside the incandescent globe, which, according to the view I have advanced, would result in the formation of various salts—sodium chloride being the principal and the most abundant.

I will now refer to the communications that have been received.

Let me say how much gratified I feel by Professor Tyndall's approval of my views. As regards Professor Prestwich's communication, which gives what may be considered the alternative theory regarding the origin of oceanic salinity, there is much to be said. But I have for a long time regarded it as an insufficient hypothesis; and as regards the statement that gypsum (sulphate of lime) "was deposited first," this is not generally the case; at least not in Worcestershire where the gypsum lies above the rock salt, nor can I admit that the Triassic strata are of marine origin in England.

Mr. Postlethwaite's letter is of very great interest to me, because it is the first case in which I have heard in the British Isles of such highly saline water as he describes being found in strata of the lower Silurian age. I do not know of any case in England, Ireland, Wales, or Scotland where these old rocks have yielded such highly saline waters as he describes. They seem to represent those which have been worked for so long a period

for chemical purposes in the United States, and this occurrence in Cumberland is very interesting, for it seems to show, as he concludes, that the waters of the lower Silurian period were really highly saline.

In conclusion I beg to thank all for the kind manner in which my paper has been received.

The Meeting was then adjourned.

#### A NOTE ON THE FOREGOING PAPER.

D. BIDDLE, Esq., M.R.C.S.E., writes:—

I feel sure that on consideration of the facts, Professor Hull's view as to the cause of the saltiness of the ocean will be found to be untenable. Although the first chapter of Genesis does not lend much countenance to the nebular hypothesis, yet in effect it states that the whole surface of the earth was fluid before the dry land appeared. Science has confirmed this testimony, and has thus assigned to the ocean the first place in mundane existence. Such being the case, it is scarcely too much to assume that the original liquid forming the ocean was capable of holding (and, in fact, did hold) in solution all those salts which are found in the ocean of the present day. It possibly held many more when its temperature was higher; and some geologists have gone so far as to assert that the solid crust of the earth is wholly a precipitate from the ocean, the stratified appearance of many rocks giving support to this hypothesis. But, be that as it may, it is not unreasonable to believe that the ocean originally held in solution all the sodium chloride entering into the earth's composition, and that so far from the land having given salt to the ocean, the reverse has been the case, the ocean having given salt to the land. One way in which this has been effected is by alterations of the earth's surface (at the first appearing of dry land and subsequently), whereby portions of the ocean have been imprisoned in basins, from which there has been no outlet except by evaporation. The salt left behind then forms part of the land, and by further alterations of the earth's surface may be overlaid, as in Worcestershire and Cheshire, or be upheaved, as in the Kalabagh Mountains of India.

Another argument against Professor Hull's view is to be found in the fact that at least one-fortieth of the ocean consists of sodium chloride, that the geographical area of the ocean is about three times that of the land, and its mean depth far greater than the mean height of the land above sea-level. Thus the salt at present in the ocean is fully equal to one-tenth of all the land from which, by gravitation, it could have been discharged, if Professor Hull's

view held good. This is, indeed, a prodigious proportion of soluble material to be even temporarily withheld from an almost omnipresent solvent. Is it not much more reasonable to believe that the ocean, as it receded from the upheaved land, took its salt with it?

The following is the author's reply :—

I do not think there is any discrepancy between my views and those of the creation of land and ocean in Genesis: on the contrary I venture to maintain they are quite consistent. Nor can I see how Dr. Biddle is to account for the sodium of the sodium chloride unless it was derived from the decomposition of the rocks by the chlorine which (I assume) existed in the original aqueous atmosphere. His statement that "it is not unreasonable to believe, &c.," is not scientific induction, but only mere assumption. As for the beds of salt in the strata of Cheshire and Worcestershire, the opinions of geologists are almost unanimous that they were formed on the beds of inland lakes like those of the Dead Sea, of Utah and of Central Asia; though, I admit, that there are instances of their formation in the way Dr. Biddle describes.

## ORDINARY MEETING.\*

PROFESSOR E. HULL, LL.D., F.R.S., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

ASSOCIATES :—Rev. C. W. A. Clarke, M.A., India; Rev. J. P. Clark, M.A., London; Rev. H. D. Buswell, Mauritius; Rev. H. J. Hoare, India.

A lecture entitled "Primitive Indian Philosophy, with some Modern Parallels," was given by Mr. W. H. Robinson,

\* 8th of 28th Session.

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ORDINARY MEETING.<sup>1</sup>

PROFESSOR E. HULL, LL.D., F.R.S., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

ASSOCIATES :—Rev. J. Cockin, Cornwall ; Commander Heath, R.N., London.

A translation, see p. 93, of the following paper was then read :—

*LA LISTE DE SHESHONQ À KARNAK.*

PAR G. MASPERO.

**L**A liste que Sheshonq I<sup>er</sup> a fait graver des villes qu'il avait ou prétendait avoir prises, pendant sa campagne contre Roboam, a été souvent étudiée par les Égyptologues. Champollion le Jeune\* et Osburn† en tirèrent tout le parti qu'il était possible à une époque où, ni le mécanisme de l'écriture hiéroglyphique, ni la géographie de la Palestine n'étaient encore bien connus. Brugsch en donna plus tard une analyse complète,‡ qui permit à Blau d'en proposer une interprétation plus sérieuse que toutes celles qu'on avait faites jusqu'alors.§ Le commentaire de Blau, modifié par

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\* Champollion, *Lettres écrites d'Égypte*, p. 99-100, *Grammaire Égyptienne*, p. 160, et *Monuments, Texte*, T. II, p. 114.

† Osburn, *Egypt, her Testimony to the Truth*, p. 158-162.







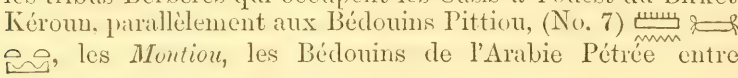
‡ Brugsch, *Geogr. Ins.*, T. II, p. 56-71.

§ Blau, *Sisaks Zug gegen Juda aus dem Denkmale bei Karnak erläutert*, dans la *Zeitschrift der deutschen Morgenländischen Gesellschaft*, T. XV., p. 233 sqq.

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<sup>1</sup> 10th of 28th Session.

Brugsch dans la seconde édition de son " Histoire d'Égypte,"\* est resté depuis lors presque classique dans la science, et la plupart des identifications auxquelles il s'était arrêtées ont été admises sans discussion par les archéologues et par les géographes.† Un premier examen, fait en 1880, m'avait montré pourtant que Blau avait pris trop de libertés avec la forme extérieure des noms, et n'avait obtenu beaucoup de rapprochements qu'au prix d'inversions et d'altérations trop nombreuses pour être légitimes: je me suis efforcé alors de prouver que les lettres égyptiennes, transcrites rigoureusement en lettres hébraïques, donnent presque partout des formes régulières de l'hébreu, et par conséquent n'exigent ni changements ni corrections.‡ J'ai voulu, dans le présent article, rassembler, après dix ans bientôt de recherches nouvelles, les résultats partiels auxquels je suis parvenu, et les soumettre, sous bénéfice d'inventaire, à la critique de mes confrères en Egyptologie. J'ai établi le texte par la comparaison de toutes les copies publiées depuis le commencement du siècle, et par la collation de la copie de Champollion avec ce qui subsiste encore de l'original sur la muraille de Karnak.§

Les dix premiers cartouches étaient remplis par les formules générales qu'on rencontre au début de plusieurs listes géographiques. Bien qu'un certain nombre d'entre eux soient détruits complètement, on peut les rétablir presque à coup sûr: (No. 1)  *To-qimâiti*, le pays du midi. (No. 2) , *To-mihiti*, le pays du Nord. (No. 3) , les tribus situées entre le Nil et la Mer Rouge, de la hauteur d'Assouân à celle de Siout. (No. 4) , les *Tuhanou*, les tribus berbères au delà des Oasis de Thébaïde, en pendant aux *Anioudu* du cartouche précédent. (No. 5) , les Bédouins, qui vivent entre le Nil et la Mer Rouge, de la hauteur de Siout aux environs de l'Ouady Toumilât. (No. 6) , les tribus Berbères qui occupent les Oasis à l'ouest du Birket Kéroun, parallèlement aux Bédouins Pittiou. (No. 7) , les *Montiou*, les Bédouins de l'Arabie Pétrée entre

\* Brugsch, *Geschichte Aegyptens*, p. 660-663.

† C'est au mémoire de Blau que Mariette a emprunté cette étrange hypothèse de corps d'armée égyptiens, manœuvrant comme feraient des corps d'armée modernes (*Les Listes des pylones de Karnak*, p. 46-48).

‡ Maspero, *Notes sur différents points de grammaire et d'histoire*, dans la *Zeitschrift*, 1880, p. 44-49.

§ Maspero, *Révision des listes géographiques de Thoutmos III*, p. 100-101.

l'Égypte et la Syrie], (No. 8) les *Paditiou*, nomades de Syrie entre les Montiou de les frontières du Naharanna,\* enfin les (No. 9) *Haïounirou* de l'Asie-Mineure et des îles de la Méditerranée. Le dernier cartouche, (No. 10) est le seul que je ne me rappelle pas avoir rencontré ailleurs. Peut-être faut-il le restituer *copie des Asiatiques*, en considérant ces mots comme une sorte de titre qui s'appliquerait à l'ensemble des noms qui suivent: ce n'est toutefois qu'une simple conjecture. Les treize cartouches qui viennent ensuite ne présentent pas non plus de grandes difficultés d'interprétation. Je me bornerai à les énumérer, avec les restitutions que j'ai cru pouvoir y joindre en 1880, et auxquelles je ne vois rien à changer pour le moment. (No. 11) *Gazatou*, Gaza, [No. 12 *Magidi*, Mageddo], (No. 13) *Rabbati*, רבית d'Issachar, (No. 14) *Tuânaqou*, תענקה, (No. 15) *Shaunamá*, שוניהם d'Issachar, † (No. 16) *Bit-Shaïnla*, *Bit-Sháilla*, où il m'a semblé reconnaître le Shiloh שילה d'Ephraïm, ‡ (No. 17) *Rouhaïba*, רהב, aujourd'hui Rehab§ au sud de Béisan, (No. 18) *Hapourouma*, חפרים d'Issachar, (No. 19) *Adoulmim* אדלמים, que ni l'orthographe hiéroglyphique ni la position qu'elle occupe dans la liste ne permettent d'identifier avec l'Odollam עדלם de Juda. Le numéro 20, qui est entièrement mutilé, renfermait peut-être le nom de Sichem, une des capitales d'Israël sous Jéroboam I<sup>er</sup>. Le No. 21 *Shaouadi* est sans doute *Souéda*, (Kharbét es-Suweidéh de la

\* Sur ces peuples voir J. de Rougé, *Textes géographiques du temple d'Édfou (H<sup>ic</sup>-Égypte)*, extrait de la *Revue Archéologie*, 1865, p. 12-16.

† L'identification a été proposée pour la première fois par Osburn, *Egypt*, p. 158.

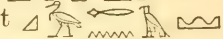
‡ Sur les noms géographiques de la liste de Thoutmôsis III qu'on peut rapporter à la Judée, dans les *Transactions du Victoria Institute*, T. XXII, p. 69-70.

§ Cfr. Eusèbe, *Onomasticon*: καὶ ἐστὶ ῥοῶβ κόμη ἀπὸ δ' σημείου Σκυθοπύλεως, ἣν δὲ Λευίταις ἀφωρισμένη (edit. Parthey, p. 316).

carte anglaise), à peu de distance de la rive droite du Jourdain.\* Les numéros suivants représentent



Mahanaïm מַחֲנַיִם au-delà du Jourdain,† et



Gabaon גַּבְעוֹן de Benjamin.‡ La présence sur la liste de villes appartenant à Jéroboam, ne prouve pas que les armées égyptiennes aient pénétré en Galilée ou franchi le Jourdain. Le roi d'Israël, en implorant l'aide de Sheshonq contre son rival, avait fait par-là même acte de vasselage vis-à-vis de l'Égypte: cela suffisait pour que ses villes figurassent à Karnak parmi les cités soumises au cours de la campagne.

Pour déterminer le site des localités qui se présentent au-delà de Gabaon, j'emploierai le procédé qui m'a servi déjà à propos des listes de Thoutmos III: je diviserai la liste en sections comprises chacune entre deux villes connues, et dont la position sur le terrain aura été indiquée de façon sinon certaine, du moins vraisemblable, par les explorateurs récents. De Gabaon à la lacune du numéro 30, le scribe égyptien a suivi la ligne de places ou de postes fortifiés qui couvraient la frontière septentrionale du royaume de Juda.

(No. 24) Bit-haouaroun est le Bethoron

בֵּית־חֲרוֹן d'Ephraïm.§ (No. 26) Aiaouloun,

l'Aialon אֵיָלוֹן de Dan,|| (No. 27) Mâkidau,

מִקְדָּה,¶ c'est-à-dire Bêt-Ou, Yalo, et Magharah مغارہ el-

\* Le déterminatif est encore très visible sur l'original et complète le mot (Maspero, *Recueil*, T. VII, p. 100). Blau (*Bp. l.*, p. 237) restituait qu'il identifiait avec عرق السودان Arak es-soudan, ou السويدان es-Souéidan de Robinson (*Palestina*, III, p. 867, II, p. 657).

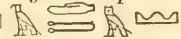

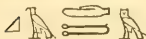
† Champollion, *Grammaire Égyptienne*, p. 160, *Monuments, Texte*, T. II, p. 114; Rosellini, *Mon. St.*, T. IV, p. 157.

‡ Brugsch, *Geogr. Ins.* T. II, p. 61.

§ Champollion, *Grammaire*, p. 160, *Monuments, Texte*, T. II, p. 114; Rosellini, *Mon. Stor.*, T. IV, p. 157.

|| Champollion, *Monuments, Texte*, T. II, p. 114; Brugsch, *G. Ins.*, T. II, p. 62.

¶ Champollion (*Mon.*, *Texte*, T. II, p. 114, et *Grammaire*, p. 160), Rosellini (*Mon. St.*, T. IV, p. 157-158), Osburn, *Egypt, Her Testimony to the Truth*, p. 160), Brugsch (*G. Ins.*, T. II, p. 62), E. de Rougé (*Mémoire sur l'origine*, p. 53), ont voulu reconnaître ici Mageddo; Blau (*op. l.*, p. 237-238) a fait observer que Mageddo serait ici hors de sa place et a proposé Makkédah, ce que j'ai accepté (*Zeitschrift*, 1880, p. 45).

Moghâr,\* à quelque distance au S.E. d'Yebuah. Cet ensemble de positions certaines nous permet de rejeter à priori les identifications proposées pour (No. 25)  *Qadoutim*, par Champollion avec עֵיטם Etham de Juda,† par Brugsch et par Rougé avec קְדֵמוֹת Kedemôth de Ruben.‡ Blau pense à la ville d'Adithaïm עֲדִיתַיִם, qui devait être quelque part dans le voisinage, et j'avais cru d'abord pouvoir accepter cette identification : la transcription  $\triangle$  Q du ע peut se justifier en effet par l'exemple de  *Qazatou*, Gaza, עָזָה.§ Il faut remarquer toutefois que, dans les noms où le ע hébraïque tend à la prononciation du غ, arabe, les versions grecques et la Vulgate ont ordinairement un γ, g, Γάζα, Γαί, Ἀγγαί = עֵי, הָעֵי, Γαδὲρ = מְגִד־לְעָרָה, Βεθφογγόρ = בֵּית־פְּעוֹר, &c.; or עֲדִיתַיִם n'est jamais transcrit Γαδιθαίμ, mais Ἀδιθαίμ, ce qui semble montrer que, dans ce mot, la lettre initiale est un ע pur sans tendance au غ, et me porte à rejeter l'équivalence de עֲדִיתַיִם Adithaim avec  *Qadoutim*. Le groupe hiéroglyphique, transcrit en lettres hébraïques, nous donne d'ailleurs une forme גְּדוּרַיִם de גְּדוּר, *incisio, sulcus, turma, agmen militum*, qui nous oblige à le considérer comme ayant été noté correctement par le scribe et par les sculpteurs de Karnak. L'équivalent moderne de ce nom serait, avec chute de la finale plurielle יַם et substitution du ח au ג, un mot *Djedid* ou *Djédoud*, qui se serait confondu infailliblement avec l'adjectif גְּדִיד *neuf, nouveau*: les *Djoudèùdèh* qu'on rencontre en plusieurs endroits de nos cartes, sont trop éloignées d'Aïalon et de Bethoron pour qu'on ait le droit de les rapprocher de notre *Qadoutim, Qadoudim*. Cette localité devait se trouver entre Bêt-Our et Yalo, probablement vers le point où la route qui réunit ces deux bourgs franchit le Ouady Selmân, et

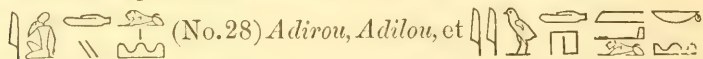
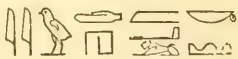
\* Le site d'el-Moghâr, proposé pour Makkédah par MM. Warren (*Palestine Exploration Fund Quart. Stat.*, 1875, p. 181) et Conder (*Quart. St.*, 1875, p. 165-167), est le plus vraisemblable de tous ceux auxquels on a songé jusqu'à présent.



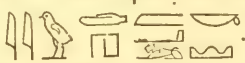
† Champollion, *Mon., Texte*, T. II, p. 114.

‡ Brugsch, *Geogr. Ins.*, T. II, p. 61; E. de Rougé, *Mémoire sur l'origine*, p. 53, 91.

§ Blau, *Sisacs Zug*, dans la *Z.d.d.M.*, XV, p. 237; Maspero, dans la *Zeitschrift*, 1880, p. 45.

de façon à barrer dans la plaine le chemin de Jérusalem; la carte anglaise porte en cet endroit un Kharbèt Bêt-Nashef qui pourrait être *Qadoutim*, *Qadoudim*.

 (No. 28) *Adirou*, *Adilou*, et 

*Jaoudhamalouk* (No. 29) ont été l'objet d'hypothèses variées et contradictoires. Comme la direction dans laquelle on a chance de rencontrer *Adirou* dépend de celle dans laquelle on trouvera *Jaoudhamalouk*, je m'inquiéterai d'abord de ce dernier nom. Champollion y avait vu l'image du royaume de Juda,\* Rosellini celle du roi de Juda.† E. de Rougé persista jusqu'à la fin à tenir l'opinion des premiers Égyptologues comme vraisemblable, sinon comme certaine.‡ Brugsch, au contraire, estima qu'on avait ici une simple bourgade dont l'équivalent moderne serait un *Yahoudiyéh*, *el-Yehoudiyéh*, soit celui des environs de *Tibnîn*, soit celui des environs de *Jaffa*.§ Blau se rangea à l'avis de Brugsch, et ajouta que notre localité, étant située au voisinage de *Makkédah*, se confondait nécessairement avec le יהוד *Iehoud* de Dan, c'est-à-dire avec *el-Yehoudiéh* voisin de *Jaffa*:|| *Jaoudhamalouk* aurait été la forme pleine, *Iehoud* une forme abrégée. Dans ces derniers temps M. Max Müller a entrepris de démontrer que l'Égyptien  ne contient pas le nom de Juda: on devrait, dit-il, retrouver dans l'Égyptien le premier ה de יהודה et avoir  au lieu de . Il pense que le terme initial du nom est יד *la main*, et que l'ensemble correspond à יד-המלך *Iad-hammelek*: la ville *Idhammelek*

\* Champollion, *Lettres écrites d'Égypte*, p. 99.





† Rosellini, après avoir adopté l'opinion de Champollion (*Mon. Stor.*, T. II, p. 79-80), fut amené par des scrupules philologiques à la modifier, et à voir dans le nom de notre localité la mention du roi de Juda (*Mon. Stor.*, T. IV, p. 158-159).

‡ E. de Rougé, *Mémoire sur l'origine égyptienne de l'alphabet phénicien*, p. 53, *Leçons de M. de Rougé dans les Mélanges*, T. II, p. 274, note 9. M. de Rougé, ou M. Robiou qui a publié les *Leçons*, attribuait à Champollion la traduction de Rosellini, et revenait au sens de *royaume*, qui est précisément celui que Champollion avait proposé.


§ Brugsch, *Geogr. Ins.*, T. II, p. 62-63.

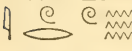

|| Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, T. XV, p. 233.


n'aurait plus rien de commun avec Iehoud de Dan.\* Malgré la très grande estime que j'ai pour M. Max Müller (de Nüremberg), et bien que M. Le Page Renouf lui ait prêté l'appui de son autorité, je ne pense pas que l'objection qu'il élève contre la lecture de Brugsch soit insurmontable.

Le mot  renferme un élément gênant, l'ou  qui suit les deux plumes  et qui n'a pas de raison d'être, si on admet la lecture יד : aussi MM. Müller et Le Page Renouf ont-ils essayé de se tirer d'affaire en expliquant qu'il ne fallait pas en tenir compte. Je pense au contraire qu'il ne faut jamais le négliger, ni dans le cas présent, ni dans les autres cas où il se rencontre. Le signe , commençant un mot, paraît avoir eu toujours un *a* pour voyelle inhérente, et cet *a*, diphthongué avec un *ou*, qui tantôt est exprimé, tantôt est supprimé dans l'écriture, a donné dans le copte


tantôt un  $\alpha$  ou un  $\epsilon$ , tantôt un  $o$ ,  $\omega$  : ,

, *iaoumá*, ים, la mer,  $\text{I}\alpha\text{E}\text{E}$ , *B.*,  $\text{I}\text{O}\text{E}\text{E}$ , *T.M.*,

, , *iaourou*, le fleuve,  $\text{I}\epsilon\text{p}\text{o}$ , *T.*,

$\text{I}\alpha\text{p}\text{o}$ , *M.*, &c. Je lis donc  *Iaoud*, non *Ioud*, et

je pense que l'hiatus de l'*a* sur l'*ou* rend l'aspirée très-faible de ה entre deux voyelles, de la même manière qu'il fait

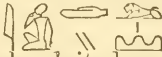
dans la transcription *Iaoudaï* des textes assyriens. 

*Iaoud* formerait ainsi la transition entre la forme pleine

יהודא of l'hébreu classique et la forme réduite 'Ioudás des

Grecs. Je conserverai, jusqu'à nouvel ordre, la lecture

*Iaoud-ha-malouk*, *Iehoud-ham-melek*, et l'identification avec

*Iehoud de Dan*, *el-Yâhoudiyéh*. Le No. 28 

*Adirou*, placé entre cette localité et *Makkédah*, doit donc

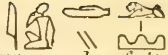
avoir eu sa place marquée dans la plaine entre *el-Yâhoudiyéh*





et *Môghar*. *Blau* prétend y reconnaître une mauvaise inter-

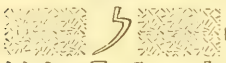

prétation égyptienne de יד, car, dit-il, une ville de l'importance de *Lydda* ne saurait manquer à la liste de *Sheshonq*. †


\* Max Müller, *The supposed name of Judah in the list of Sheshonq*, dans les *Proceedings of the Society of Biblical Archaeology*, T. X, p. 81-83 à la suite de ce mémoire, on trouve (p. 83-86) des *Remarks* dans lesquelles M. Le Page Renouf approuve les conclusions de M. Max Müller.

† *Blau*, *Sisags Zug*, dans la *Z.d.d.M.*, T. XV, p. 231.


Brugsch avait fait observer très justement que  rend lettre pour lettre un dérivé de la racine **אָדַר**, *amplus fuit*, sans doute **אָדִיר**, *amplus, potens*.\* Ce nom *Addirou*, *Addir*, présente une consonnance si proche de celle de l'arabe **الدير** *al-dêir, ed-dêir*, qu'il a dû se confondre avec l'un des nombreux *dêir* qui figurent sur nos cartes modernes. Donné la position qu'il occupe sur la liste, je suis tenté de le reconnaître dans le **خربة ديران** *Kharbét-Dêirân* que la carte anglaise signale à quelque distance au nord d'el-Môghâr.

Le cartouche No. 30 est détruit à l'exception du signe  qui ne nous apprend rien. La série qui recommence au-delà, et qui est interrompue après le No. 40 par une longue lacune, nous offre vers la fin un nom (No. 38)  *Shaouka*, qui est écrit  *Saouka* sur la liste de Thoutmos III (No. 69): c'est la *Shokoh* de la plaine judéenne, aujourd'hui *Kharbét-Shouwêikéh*.† Les noms compris entre *Iaoud-ham-melek* et *Shaouka* doivent donc s'échelonner plus ou moins régulièrement entre *el-Yahoudiyéh* et *Kharbét-Shouwêikéh*. De plus le No. 35, qui est mutilé 

 se laisse rétablir avec certitude en  *Iaouhana*, qui est le (No. 68)

 *Iouhamâ, Iaouhmâ*, des campagnes de Thoutmos III: or, *Iaouhana* est, comme l'a montré Saulcy,‡ *el-Khêiméh* **الخيمه**. Cette restitution nous permet de diviser la série en

deux sections, une de quatre noms de *Iaoud-ham-melek* à *Iaouhana*, une de cinq de *Iaouhana* à *Shaouka* et au-delà. Les quatre noms qui composent la première section ne sont pas malheureusement d'une identification aisée. Le No. 31

 *Hianim, Hianim* se présente à nous comme une forme pluvielle **חַיִּים** d'un mot dérivé de la racine **חָנַן** *propitius fuit, favit alicui*; il est identique pour la signification au **בֵּית־חָנָן** de Dan ou Juda, et je suis disposé à le rapprocher du *Kharbét-Hannounâh* **خربة حنوبه**, situé dans le

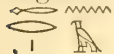
\* Brugsch, *Geogr. Ins.*, T. II, p. 62.

† Brugsch, *Geogr. Ins.*, T. II, p. 64.

‡ F. de Saulcy, *Lettre à M. Chabas*, dans les *Mélanges d'Archéologie Égyptienne*, T. I, p. 122-123.




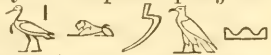
collines à l'Est d'el-Yahoudiyéh,\* et auprès duquel la carte anglaise marque des ruines.† Le nom suivant (No. 32)



*Alana, Alouna* se transcrit en lettres hébraïques

עֵלֹן, עֵרֹן, et, par conséquent, ne saurait être ni l'Elon אֵילֹן de Dan, ni Eglon עֵגְלוֹן, comme le voudraient Brugsch‡ et Blau:§ nous avons ici une localité portant le même nom

que la  de Thoutmos III, où j'ai reconnu une variante de l'hébreu עֵלְיוֹן, *superior, suprema, summus*. La

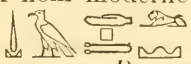
nomenclature contemporaine nous donne plusieurs עלין Alin, dont l'orthographe s'accorde parfaitement avec celle de notre ville. La seule dont le site puisse à la rigueur nous convenir est celle que la carte anglaise appelle Kharbét Ouady Alin خربة وادي علين, et qui est au Nord de Shouwèikéh, près d'Ain Shems, où l'on place ordinairement la Beth-Shemesh de Juda: toutefois le saut serait trop grand entre Kharbét Hannounah et Kharbét Ouady Alin pour que j'ose rien affirmer à cet égard. Brugsch lit 

*Biloma* le numéro 33 et l'identifie à בִּלְעָם Bilcam de Manasséh, malgré l'absence de ע dans la transcription égyptienne.‖ De fait Champollion, Rosellini et Lepsius donnent tous




*Biromam, Biloman*, et c'est la leçon

qu'on déchiffre encore sur la muraille. *Biroumim* ou *Biloumim*, est en effet le pluriel régulier d'un mot provenant, soit de la racine בָּלַם *constrinxit, clausit*, soit de la racine בָּרַם *frenuit*:

בְּרוּמִים, qui marque le nom d'une sorte d'étoffe brodée, le reproduit lettre pour lettre. Je ne trouve malheureusement sur aucune carte, aucun nom moderne qui rappelle celui de *Biroumim* ou *Biloumim*.  (No. 34) *Zaidipoutir*

ou *Zadipoudil* nous est connu au *Papyrus Anastasi* No. 1, sous





\* Guérin, *Samarie*, T. II, p. 74.


† Osburn (*Egypt, her Testimony*, p. 160) a rapproché Haianim de הַיְהוּזִים la vallée de Hinnom. Sans parler des difficultés topographiques que présente cette identification,  ne répond jamais au הָ hébraïque.



‡ Brugsch, *Geogr. Ins.*, T. II, p. 63.

§ Blau, *Sisags Zug*, dans la *Z.d.d.M.*, T. XV, p. 235.

‖ Brugsch, *Geogr. Ins.*, T. II, p. 64.

la forme  *Zidipouti* ou *Zaidipouti*, avec chûte de  R, L, finale.\* Le nom est évidemment formé de deux mots. Le premier se rattache à la racine צוּר, *venari*, et d'après la vocalisation en װ de l'Égyptien, répond plus particulièrement à צוּר *venatio, fera venando capta, cibus, commeatus*: c'est la même origine que celle de la Sidon צוּרִין des Phéniciens. Le second nom peut-être comparé à diverses racines, פָּנַר d'où פָּנַר *adeps*, פָּטַר *fidit*, פָּתַל *contorsit*, נִפְתַּר *interpretatus est somnium*, entre lesquelles je laisse au lecteur le soin de choisir ce qui lui conviendra le mieux. Ces noms composés s'abrègent de toute manière: si l'on admet que le second terme  *poutir* soit tombé, le premier 

 *Zaïdi*, rappelle aussitôt le bourg de صيدون Saïdoum, Sîdoum, qui est situé un peu au Sud de Tell-Djezer, et au Nord de Kheïmèh. Guérin y signale quelques débris qui lui font supposer en cet endroit l'existence "d'une antique bourgade que l'histoire ne mentionne pas, et qui, à la différence de la grande ville de Sidon, son homonyme, est demeurée toujours obscure et probablement sans importance."† L'emplacement de Saïdoum convient à la position que Zaïdi-poutir occupe auprès de Iaouhamâ.

Brugsch lisait pour le No. 36  *Bit-âlemat*, et proposait d'y reconnaître עֲלָמִית *Allemet*, עֲלָמִיִן *Almôn* de Benjamin.‡ aujourd'hui 'Almît au N.E. de Jérusalem. La lecture et l'identification ont été acceptées par Rougé.§ Blau de son côté préfère transcrire בֵּית-עֲלָם *Bêth-Olam*,|| et se déclare pour la Beit-alam بيت علام de Robinson.¶ Comme les copies de Champollion, de Rosellini et de Lepsius portent  *Bi-âlamim*, j'avais cru pouvoir re-

\* *Papyrus Anastasi I*, pl. xxii, l. 5; Chabas, *Voyage d'un Égyptien*, p. 199, a songé le premier à comparer le nom de la liste de Sheshonq à celui du Papyrus Anastasi. Il ne propose aucune identification.

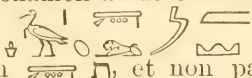



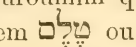
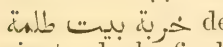



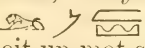











† Guérin, *La Judée*, T. II, p. 33.

‡ Brugsch, *Geogr. Ins.*, T. II, p. 64.

§ E. de Rougé, *Mémoire sur l'origine de l'alphabet phénicien*, p. 95.

|| Blau, *Sisags Zug*, dans la *Z.d.d.M.*, T. XV, p. 238.

¶ Robinson, *Biblical Researches in Palestine*, T. II, p. 403; cfr. Guérin, *Judée*, T. II, p. 369.






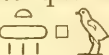
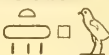
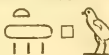


pousser les deux hypothèses.\* L'examen du mur de Karnak m'a montré qu'il fallait écrire  *Bitoumim* ou *Bitou-Roumim* avec un  et non pas un . † On ne peut songer à une lecture Bit-Touloumin ou Bit-Touroumim qui nous donnerait un nom équivalent soit à la Telem  ou  *Telaïm* de Juda, soit au Tolmah  des cartes modernes. ‡  est ici une variante de la finale , *ti, t* de  *Bit*, et indiquent une vocalisation en *o* analogue à celle que la prononciation *Bethogabra, Betogabris*, nous révèle. La seconde partie du nom  *rounam, loumam, ou roumim, oumim*, peut-être, soit un mot singulier comme  *elatio*, soit un pluriel,  de  *bubalus*, ou d'un dérivé de la racine  *altus fuit*. *Bit-roumim* est un nom correct, mais que je ne sais où placer sur le terrain.  (No. 37) *Qaqali, Qaqari*, est pour  *Kéilah* de Juda, qu'on identifie avec la *Kharbét Kilâ* de l'Ouady Souwêidéh, à l'Est de Beit-Djibrin. Le rapprochement de *Qaqali, Qaqali* avec *Kéilah* me paraît être légitime, car la transcription *Kegila* de la Vulgate montre que le *ע* avait dans ce nom la tendance au *غ*; il pouvait être rendu par  $\Delta$  en Egyptien, comme le *ע* de Gaza l'est par  $\Delta$ . Si l'on trouvait que le site de *Kéilah* est trop au Sud, on pourrait songer à un autre *Kharbét Kila*, que la carte anglaise signale à l'Ouest d'Eshouâ, et dont l'emplacement au Nord de Shouwêikéh et à l'Est de Khéiméh est en accord suffisant avec la position de *Qaqali* entre Iaouhamâ et Shaouka. Les deux noms qui succèdent à celui de Shaouka (No. 39)  *Bit-tioupon, Bit-Toupon*, et (No. 40)  *Abila*, ont déjà été signalés sur la liste de Thoutmos III avec l'orthographe (No. 98)  *Tiouponou, Tipounou* et (No. 99)  *Aoubilou*, dans le même ordre où les a rangés le scribe de Sheshonq. Le retranchement ou l'addition de  *בית*

\* Maspero, dans la *Zeitschrift*, 1880, p. 46.

† Maspero, dans le *Recueil*, T. VII, p. 100.

‡ Guérin, *Judée*, T. I, p. 256.

§ Blau, *Sisags Zug*, dans la *Z.d.d.M.*, T. XV, p. 238.

est un fait trop fréquent dans la nomenclature juive pour nous étonner ici, et la finale  *nou* se rencontre dans  *Loudni*, par exemple, pour *Loud*. J'ai rejeté déjà la conjecture de Brugsch, *Bit-Tipounou* = Beth - Tappouah, .\* Blau ne l'avait admise qu'après que Brugsch lui eût assuré avoir retrouvé sur la pierre l'équivalent du  final.† J'ai constaté que ce  n'existait pas réellement et que le mur portait  non  ou  et l'absence de cette lettre suffit pour rendre la comparaison impossible entre les deux noms. Le rapprochement avec *Téphôn*, *Topho* du premier livre des Maccabées IX, 50, n'est admissible qu'à la condition que cette Téphôn inconnue ne soit pas une forme grécisée de Beth-Tappouakh, Tappouakh.‡ Si le nom de  Dêr Doubbân § pouvait être considéré comme provenant d'un caprice d'étymologie populaire, qui aurait substitué le mot  à un nom ancien, *Bit-Toupou*, *Toupounou* pourrait avoir occupé le site que les fellahs appellent de la sorte. L'Abila du No. 40 a disparu.

La grande lacune qu'on remarque après Abila, ne compte pas moins de douze cartouches, dont deux seulement prêtent à restitution. Brugsch avait d'abord complété (No. 46)  en  *Bit-T'abouti* ;|| Blau songea ensuite à rétablir  *Bit-T'abiro* qu'il affirma être  Beth-Zour de Juda.¶ Toutefois, si l'on a parfois un  *oua* où l'hébreu met un , on n'a

\* Maspero, *Sur les noms de la liste de Thoutmos III qu'on peut rapporter à la Judée*, dans les *Transactions du Victoria Institute*, T. XXII, p. 167-68.



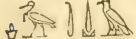
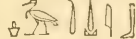
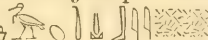





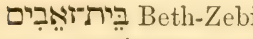

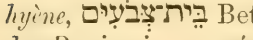
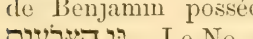








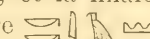

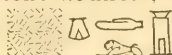
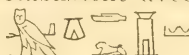
† Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, T. XV, p. 238. "Das schliessende *h'* nach Brugsch's mündlicher Mittheilung wirklich von ihm nachträglich auf dem Steine gefunden worden ist."

‡ *Maccabées* I, ix, 50. De même pour le nom de *Καφαρόφα* que Rufin paraît avoir lu dans *Joséphe (de Bello, V, 4)* au lieu de *Καφαρόφα* (Reland, *Palästina*, T. II, p. 692). Neubauer (*Géographie du Talmud*, p. 112) tend à y reconnaître Tappouakh, ce qui m'empêche de le rapprocher de notre *Bit-Toupou*.



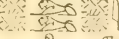
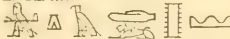

§ Robinson, *Biblical Researches*, T. II, p. 2-354, 421, qui incline à reconnaître en cet endroit le site de Gath-Rimmon; cfr. Guérin *Judée*, T. II, p. 104-6.


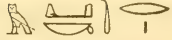

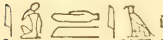
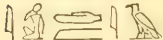



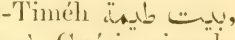
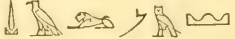


|| Brugsch, *Geogr. Ins.*, T. II, p. 65.

¶ Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, T. XV, p. 238-9. Au témoignage de Blau, Brugsch aurait adopté l'identification avec Beth-Zour.

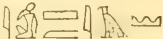

jamais un  pour un  hébreu, et ביתצור serait transcrit  non . Les copies de Champollion, de Rosellini et de Lepsius diffèrent légèrement de celle de Brugsch, et j'ai pu vérifier moi-même que le texte original porte .\* La seule restauration possible est celle d'un M diversement écrit et diversement vocalisé, soit  soit  soit  *Bit-Zabima*. Le mot  *Zabim* est un pluriel régulier, pouvant venir soit de  *loup*,  *Beth-Zebim*, *la maison des loups*, soit même de  *hyène*,  *Beth-Zeboïm*, *la maison des hyènes*: la tribu de Benjamin possédait de même une vallée des hyènes . Le No. 47 se présente sous la forme  où il ne manque qu'un signe long entre le  et l'aigle . En rétablissant  on aurait l'équivalent de  *stella*, mais cette restitution si séduisante qu'elle soit est invraisemblable; le  n'est jamais suivi de  dans notre liste, et la finale  y est au contraire si fréquent que la lecture  *Koka* s'impose à nous. Le nom répond à l'hébreu  *qana*, *tectum*, *superficies altaris* et s'appliquerait fort bien à un village situé en terrasse sur le sommet d'une colline. Je ne ferai aucune conjecture sur l'emplacement de ces deux localités; la lacune qui les enveloppe est trop grande pour qu'on puisse déterminer même la direction dans laquelle on doit les chercher. La liste reprend avec le No. 52 et continue, non sans mutilations, jusqu'au No. 58. Les deux derniers numéros 57 et 58 ont légèrement souffert, mais se rétablissent avec certitude: le No. 58  est 

\* Maspero, dans le *Recueil*, T. VII, p. 100.

† Champollion (*Monuments, Texte*, T. II, p. 116) porte , Rosellini (*M. St.*, pl. cxlviii), , Lepsius (*Denkm.*, III. bl. 252), Brugsch (*Geogr. Ins.*, T. II, pl. xxiv), . J'avais reconstitué de toutes ces copies la lecture  (*Zeitschrift*, 1880, p. 46); la muraille de Karnak porte  (Maspero, dans le *Recueil*, T. VII, p. 100), qui prouve que la restitution était exacte.

Magdilou, et le No. 59  Iarza.\* Les deux noms figuraient déjà sur les listes de Thoutmos III,  Magdilou (No. 71) et  (No. 60), et sont aujourd'hui el-Medjdel et Kharbét Ierzah. Il y a donc grand chance pour qu'une partie au moins des noms qui précèdent immédiatement ces deux-la aient appartenu à des localités voisins de Kharbét Ierzah et d'el-Medjdel. Le No. 56  Adima † a été identifié par Brugsch avec le pays d'Edom, ce à quoi Rougé ne répugne pas.‡ Le rapprochement, irréprochable au point de vue philologique, tombe de lui-même quand on se rappelle que nos listes ne contiennent pas des noms de pays étendus, mais simplement la désignation d'accidents de terrain, de villes ou de villages.  Adouma doit donc indiquer ici un bourg qui s'appellent  la rouge ou  le champ, comme une des bourgades de la tribu de Naphthali, ou comme une des cinq villes maudites . Le seul nom moderne qui puisse être mis à côté d'Adouma, est celui de Bêit-Timéhi  بيت طيمة, gros village situé à l'Est de Ierzah, et où Guérin signale l'existence de ruines romaines ou byzantines:§ le site en conviendrait fort bien à la position relative des noms dans la liste et l'orthographe moderne peut à la rigueur se déduire de l'orthographe ancienne. Le No. 58  Zaloumim, est un pluriel du mot  *umbra*, mais ne peut être identique à la station  Zalmonah de l'itinéraire des Hébreux dans le désert, comme le voudrait Brugsch: || le voisinage de noms comme Migdol et Ierzah ne nous autorise pas à descendre si loin dans le sud, en cet endroit de notre liste. On ne trouve plus sur le terrain aucun nom qui rappelle celui de Zaloumim, mais le mot lui-même est expressif, et peut rappeler les ombrages des jardins qui


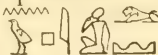
\* La restitution Ierza, que j'avais proposée en 1880 (*Zeitschrift*, 1880, p. 46), a été confirmée par l'examen du texte original (*Recueil*, T. VII, p. 100).






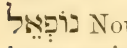
† Champollion, *Monuments, Texte*, T. II, p. 116) lisait  (cfr. *Zeitschrift*, 1880, p. 46) : la muraille porte réellement *Adima*, par un , comme Brugsch le disait (*Geogr. Ins.*, T. II, pl. xxiv).



‡ Brugsch, *Geogr. Ins.*, T. II, p. 66-67; E. de Rougé, *Memoire sur l'origine de l'alphabet phénicien*, p. 53.



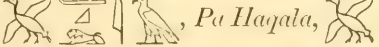

§ Guérin, *Judée*, T. II, p. 127-128.


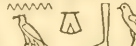



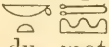
|| Brugsch, *Geogr. Ins.*, T. II, p. 67.

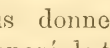

entourent el-Medjdel: un site comme celui du village de Hammânéh  conviendrait au peu que nous devinons de Zaloumim, Zelemim. Les trois numéros qui précèdent ne devaient pas être situé bien loin d'el-Medjdel. 


Noup-îlou (No. 53) est composé de la finale , El, Dieu, et d'un dérivé de la racine  *agitavit*, ou  *eminentia*,  *locus*, *editus*. La ville de  Neballat, Beit-Nebâla, dans la tribu de Benjamin, que Blau reconnaît ici, ne répond à  Nough-îlou, ni par l'orthographe ni par la position :\*

je ne vois d'ailleurs, entre Shouëikéh et el-Medjdel, aucun nom qui semble provenir du nom antique et se prête à lui être assimilé.  (No. 54) Dishati, Doushati, est malgré, sa tournure exotique, un nom sémitique. C'est une forme féminine, dérivée de la racine  *terere*, *conterere*, *triturare frumentum*; mais cela dit, je ne vois aucun moyen de placer la ville sur le terrain. Le numéro 55 est non-seulement difficile à localiser, mais difficile à déchiffrer. Je le

lis comme Champollion .† Il renferme l'article masculin égyptien  *pa*, dont nous avons nombre d'exemples dans , *Pa Haqala*, 











*Pa âmaqou*,   *pa nagabou*. Le signe suivant est bien le signe  *oirou*, le Grand, le chef, qui se rencontre presque aussi souvent sans son  final qu'avec son  dans les textes hiéroglyphiques.  transcrit en









hébreu nous donne  pluriel du mot  *toroular*. Osburn a proposé de traduire le tout par *Le prince des Gadites*,‡ et en effet aucune raison philologique ne s'oppose à ce qu'on traduise comme lui les deux premiers mots; seulement le dernier ne désigne certainement pas les gens de Gad qui n'ont rien à voir en cet endroit. Il serait très agréable de traduire le chef de Gath en toute sécurité, mais il ne me

\* Blau, *Sisags Zug*, dans la *Z.d.d.M.*, T. XV, p. 240. Brugsch, après avoir reconnu l'étymologie réelle (*Geogr. Ins.*, T. II, p. 65), tendait à reconnaître dans Noup-îlou un équivalent de  Pnuel, ce qui ne répond ni à l'orthographe du nom, ni la place qu'il occupe sur la liste de Sheshonq.

† Champollion, *Monuments, Texte*, T. II, p. 116.

‡ Osburn, *Egypt, her Testimony to the Truth*, p. 162.

semble pas que *Kitout* puisse être  $\text{קִית}$ . Cette interprétation ne nous tire pas, comme on voit, de nos difficultés; elle a même l'inconvénient de soulever une objection très forte. Les chefs des pays vaincus ne figurent jamais dans les listes: le cartouche No. 54 de Sheshonq serait un cas unique dans l'archéologie égyptienne, s'il fallait le prendre pour ce que Osburn a vu en lui. Mon impression est donc que nous ne devons pas ici diviser le groupe: il faut le considérer comme formant un seul mot *Pouroukit*, *Paouroukit*, que le scribe s'est amusé à écrire avec des éléments donnant en Égyptien un sens qui n'a rien de commun avec la signification originale en hébreu. Il a transcrit la première syllabe *Pour*  , *paourou* le chef, au lieu de   par exemple; il a choisi pour la seconde, *kit*, le terme  qui est une variante graphique de   $\text{קִית}$  *kiti*, qui signifie *autre*, et le nom *Pouroukit*, s'est trouvé analysé en une phrase égyptienne plus ou moins correcte qui veut dire *l'autre chef*.     *Pouroukiti*, *Pouloukiti* est pour moi le mot  $\text{פֹּלְגוּת}$  de  $\text{פֹּלְגָה}$ ,  $\text{פֹּלְגָה}$  *rivus*, en arabe  $\text{فَلَج}$ ,  $\text{فَلَج}$ . Nous avons en effet dans le canton d'el-Medjdel un bourg de فالوجا Faloudja,\*  $\text{الفالوجي}$  el-Faloudjy, qui, pour le nom et la position, me paraît convenir à notre Pouloukiti.

Une nouvelle lacune de quatre cartouches interrompt de nouveau la série. Le numéro 64    ... *papen*, a été complété par Blauf en 'Aapen,  $\text{עֶפְנִי}$  Ophni, Gophmah: la restitution ne tient pas compte de la syllabe médiale *pa*, et ne se défend pas, mais je ne sais, non plus que Brugsch, § comment rétablir le groupe. Par bonheur, le numéro 65 nous fournit une indication précieuse. Brugsch a reconnu que      *Aazamu*|| répondait soit à l'Azem  $\text{עֶזְמִן}$  de Juda, attribuée ensuite à Siméon,¶ soit à  $\text{עֶזְמִן}$

\* Guérin, *Judée*, T. II, p. 124.

† Robinson, *Biblical Researches in Palestine*, T. II, p. 421, T. III, *App.*, p. 209.

‡ Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, T. XI, p. 241.

§ Brugsch, *Geogr. Ins.*, T. II, p. 67.

|| Brugsch, *Geogr. Ins.*, T. II, p. 68.

¶ Josué xv, 29; xix, 3.



l'Azmon, qui marque la limite méridionale du territoire juif.\* Son opinion fut rejetée par Blau, qui préfère Etham עֵיטָם,† mais Rougé‡ se prononça pour Azem, qui répond en effet plus exactement qu'Azmon à l'orthographe hiéroglyphique. L'identité des deux villes est certaine, mais la position d'Azem est tenue pour incertaine par la plupart des auteurs qui se sont occupés de la Bible. La façon dont le texte de Sheshonq introduit Azama à peu de distance de Migdol-Gad (el-Medjedel) et d'Iourza (Kh. Ierzah), nous oblige à écarter les hypothèses de ceux d'entre eux qui placent Azem très loin vers le Sud, celle de Wilton, par exemple, qui ne veut reconnaître qu'une seule localité dans les noms Ijim et Azem du livre de Josué, et la met à el-Aujéh dans le territoire des Arabes Azâziméh. Il est probable que cette ville devait se trouver dans la partie la plus septentrionale de Siméon, entre le Ouady el-Hesî et le Ouadi esh-Sheriâh, plus près du premier que du second, puisque la liste égyptienne n'intercale que cinq cartouches entre Iourza et Azama. La place précise en est d'autant plus malaisée à déterminer que, pour rencontrer une ville dont l'emplacement ne prête sujet à aucun doute, nous devons descendre à plus de quarante cartouches plus bas, à l'Arad (No. 108) cananéenne. Là du moins nous nous retrouvons un moment sur un terrain solide : עָרָד Arad est le Tell-Arad de nos cartes modernes.§ Si nous réfléchissons que notre liste consiste en plusieurs séries de noms représentant des sites peu éloignés l'un de l'autre, nous serons portés à croire que ces quarante et quelques cartouches, de valeur indéterminée pour l'instant, doivent être disséminés, de façon plus ou moins régulière, sur le pays qui s'étend des environs de Ierzah jusqu'à ceux d'Arad. La présence, dans les sections précédentes, de plusieurs localités sises assez près l'une de l'autre au Nord de l'Ouady el Hesy, me porte à penser que, les premiers au moins de ces quarante cartouches doivent être recherchés, soit sur le cours même de cet Ouady, soit dans le massif de collines qui longe sa rive méridionale. D'autre part, l'absence de noms tels que Gérar et Ber-Sheba semble prouver que l'aire géographique de la liste ne s'étend pas très loin vers le Sud. Le canton à explorer n'est donc pas aussi considérable qu'on serait tenté de le croire au premier




\* Nombres xxxiv, 4 ; Josué xv, 4.

† Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, XV, p. 241.

‡ E. de Rougé, *Mémoire sur l'origine*, pp. 77, 95.

§ Brugsch, *Geogr. Ins.*, T. II, p. 70.


abord : malheureusement il est dans une partie encore imparfaitement connue, malgré les beaux travaux du *Palestine Exploration Fund*, et les cartes y laissent subsister des espaces presque entièrement vides de noms ou d'indications topographiques. Les identifications y ont donc un caractère d'incertitude plus grand encore que celui qu'elles présentent dans les régions étudiées précédemment.


A bien considérer les choses, les quarante et quelques cartouches sont loin de représenter autant de localités indépendantes. Beaucoup d'entre eux contiennent, comme l'a vu Brugsch, des noms communs qui servent à désigner des accidents de terrain. Ces mots, précédés quelquefois d'un article masculin  *pa* ou féminin  *ta*, forment la première partie d'un nom, dont la seconde partie est inscrite dans un autre cartouche, avec ou sans intervention de la préposition égyptienne  *n*. Le plus fréquemment employé


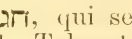

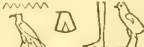


de ces mots est, avec des orthographes diverses, 

(No. 71) *Hougra*,  (No. 77) *Haqra*, 

(No. 94) *Hagri*, une fois même au pluriel ou au duel

 (No. 107) *Haqrîma*, *Haqraïma*. Brugsch

y a vu le mot arabe  *pierre*,\* et j'ai adopté son interprétation.† Elle soulève une objection des plus graves :

 est arabe, et nous sommes en pays hébreu. Le seul équivalent légitime du mot égyptien serait  *חגרה*, qui se trouve dans quelques passages géographiques du Talmud, et auquel on attribue le sens *enceinte*, *mur*, de la racine  *chavêt*.‡ On pourrait entendre ce mot de ces grandes enceintes de pierre dont beaucoup subsistent encore dans l'Arabie Pétrée, et dont on a retrouvé plus d'un vestige dans les parties du pays où la liste de Sheshonq nous conduit.§ Le second mot  *nagabou* est comparé par Brugsch soit au  *Negeb*, hébreu, soit au 




*nakb*, ou passe des Arabes. La même objection qui a été

















\* Brugsch, *Geschichte Ägyptens*, p. 661-662.












† Maspero, dans la *Zeitschrift*, 1880, p. 47.











‡ H. Hildesheimer, *Beiträge zur Geographie Palästinas*, p. 67, sqq.

§ En voir une description très claire dans Palmer, *The Desert of the Exodus*, T. II, p. 320 sqq.

soulevée contre  vaut également contre , et c'est réellement  qui seul peut répondre au terme égyptien.

    $\Delta$  *âmaqou* est  la vallée;    | *ta shodinaou* est un terme égyptien qui semble signifier *les canaux, les fossés*. Quant à     *Shabbalout*, où Brugsch avait cru reconnaître  *la plaine*,—ce qui est impossible,  répondant à , jamais à ,—c'est  *fluxus aqua, flumen*, un torrent.\* Tous ces mots sont en accord parfait avec la nature du pays où étaient placées les localités qu'ils servent à désigner: de même que les noms que les listes de Thoutmos III nous font connaître, Abilou, Karmona, Ganotou, nous montrent la fertilité du territoire central de Juda,† ceux de la liste de Sheshonq témoignent de ce qu'était le territoire de Siméon.

Du moment qu'on tient compte des observations qui précèdent, le nombre des sites à chercher se restreint sensiblement: du numéro 65 au numéro 110 il tombe à trente sur quarante cinq cartouches.‡ Les voici dans l'ordre même où Sheshonq nous les a livrés. (Nos. 65-66)            *הַעֲמֶק עֵינָם*, la Vallée d'Azama, c'est-à-dire probablement la vallée où était situé le bourg d'Azama et

le bourg lui-même: site inconnu, (No. 67)      *Anara* ou *Anala*, non *Anali* ou *Anari*, car la façon dont  est gravée me fait croire que le sculpteur, ne s'étant pas réservé une place suffisante pour le  final, s'est contenté d'insérer  devant . Ce nom est d'ailleurs un de ceux,—ils sont assez rares,—dont je ne retrouve pas l'équivalent sémitique: peut-être contient-il une faute et devrions-nous lire *Alouna*, qui nous donnerait une forme régulière  *quercus*.








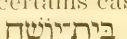








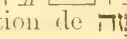
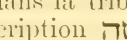


De toute façon le site reste inconnu. (Nos. 68-69)   

\* Maspero, dans la *Zeitschrift*, 1880, p. 47.

† Voir quelques observations à ce sujet dans les *Transactions of the Victoria Institute*, T. XXII, p. 67 et 74.

‡ A partir de ce moment je ne citerai plus Blau que rarement: comme il prenait chaque cartouche pour un nom complet, la répétition perpétuelle des mots, dont je viens de parler, l'a induit en presque autant d'erreurs qu'il reste de cartouches dans la fin de notre liste.

 Pa hagra-fit-iaousha.

Le nom présente une particularité assez curieuse.  y est pour  par substitution du , v. f. au  : nous verrons plus loin que le  est transcrit  dans plusieurs mots. Ces variantes ne sont pas dues au caprice du scribe égyptien : je pense qu'elles nous révèlent un fait de phonétique nouveau. Il semblerait, d'après elles, que, dans l'hébreu des gens de Siméon, le  était affaibli en *v, f*, au moins dans certains cas. *Fit-iaousha* est donc la transcription exacte d'un  *Béth-Ioshah*, dont le premier élément est légèrement défiguré par une prononciation *Vit, Fit*, pour *Bit*. Le terme joint à  n'est pas, comme je l'avais cru,  *Jen* : la coupe  *Fiti*  *aousha*\* que suppose cette identification n'est pas admissible dans notre liste, où  est toujours écrit sans  final. Je coupe  *Fit*  *Iousha*, et je prends *Iaousha* pour la transcription de  *Ioshah*. Ce nom désigne, dans le premier livre des Chroniques,† un des chefs Siméonites qui, au temps d'Ezéchias, émigrèrent à l'Orient de la ville de Guédor, cherchant des pâturages pour leurs troupeaux. Quelque soit la valeur du renseignement fourni par les Chroniques, il nous montre du moins que le nom de *Iaousha* était usité dans la tribu de Siméon, et confirme la légitimité de la transcription  *Béth-Ioshah* que j'ai adoptée pour *Fit-Iaousha*. Pour retrouver dans la nomenclature actuelle un équivalent de cette localité, il convient de ne pas oublier que le phénomène dialectal saisi par l'oreille des contemporains de Sheshonq a dû se perpétuer chez toutes les populations qui se sont succédées dans ces parages : un nom, une fois pris dans une prononciation, passe avec cette prononciation aux générations différentes qui ont à s'en servir journallement. L'équivalent arabe du  initial doit donc être quelque chose d'analogue à l'Égyptien  *rit*, *fit*, soit *فيت* ou *فت*. Je trouve en effet, un peu au sud de l'Oued esh-Sheriaï, un oued et une ruine que Guérin appelle *واد فتيس* *Oued Ftis* et *خربة واد الفتيس* *Kharbét Oued*

\* Maspero, dans la *Zeitschrift*, 1880, p. 47.

† 1 Chroniques iv, 35.

el-*Atis*,\* et que la carte anglaise orthographique Kharbét Futéis. فتيس renferme tous les éléments de : y est la contre partie de = בית, et يس répond à יושה par cette substitution du au qui est si fréquente dans la passage à l'arabe des noms hébreux. L'identification de Fit-Iaousha avec Kharbét Foutéis, Fatéis ou Ftís confirme l'opinion que j'exprimais plus haut au sujet d'Azamah, et m'encourage de plus en plus à chercher cette ville entre l'Ouady el-Hesy et l'Oued esh-Sheriah, à peu près sur une ligne menée de Kharbét Foutéis à el-Medjdel ou à Kharbét Ierzah.

C'est également dans le voisinage de Kharbét-Foutéis qu'il conviendrait de chercher les bourgs qui suivent, si les cartes n'étaient d'une pauvreté désespérante. Arouhaloul, Alou-haloul, est un de ces noms où Rougé voudrait rencontrer l'article arabe ال ; † j'ai déjà dit plus haut, ‡ combien il était difficile d'admettre dans notre liste la présence de formes arabes et je n'insiste pas. Je pense que nous avons ici un de ces noms אֶל-הַלָּל *El-halal*, où entrent et le mot אֵל dieu, et la racine הָלַל *luxit, splenduit*. Le site m'en est inconnu.

(Nos. 71-72) *Pa hougra* *abilama* contient un pluriel אֲבָלִים de אֲבָל *pré, prairie* : le tout se traduirait *l'Enceinte des prés*, et désigne un site inconnu.

*Shabbalout ni Gabri* (Nos. 73-74) signifie littéralement *le fleuve du Héros*, car *Gabri* est la transcription exacte de גַּבְרִי *vir, miles*. Blau avait identifié le second cartouche à Betogabris, qui est aujourd'hui Beit-Djibrin, § et je m'étais rangé à son avis, || mais Beit-Djibrin est trop éloignée du pays où le voisinage de Kharbét Foutéis nous oblige à rester, pour qu'il soit permis de persister dans cette opinion. J'avais songé un moment à Azion-Gaber עֲצִיּוֹן-גַּבְרִי, mais cette ville est trop

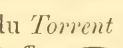
\* Guérin, *Judée*, T. II, p. 287.


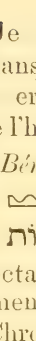
† E. de Rougé, *Mémoire sur l'origine*, p. 90

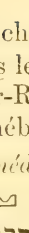
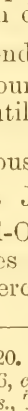
‡ Voir plus haut, p. 18-19.

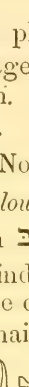
§ Blau, *Sisaks Zug*, dans la *Z.d.d.M.*, T. XV, p. 23. Il lisait le nom *Ngbari*, "wobei das n, entweder bloss lautlicher Vorschlag ist, oder graphisch richtiger am Ende der Cartouche zu stellen sein wird."

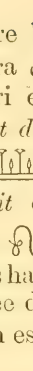
|| Maspero, dans la *Zeitschrift*, 1880, p. 47.

loin vers le Sud. Je chercherai plutôt, notre bougade de Gabri et son *fleuve* dans le voisinage d'el Gabra *الجابري*, un peu à l'Est d'Oumm er-Roumanin. El-Djabri est en effet l'équivalent arabe de l'hébreu *גַּבְרִי*. *Le torrent du Héros* est suivi du *Torrent des Bénédiction* (Nos. 75-76)  *Shabbalout - Ouarakit* où l'hébreu *בְּרָכָה*, *בְּרִיחַת*, *בְּרִיחַת*, pluriel *בְּרִיחַת*, a son ב transcrit *oua*, selon la prononciation dialectale que j'ai indiquée plus haut.\* Cette localité est certainement différente de la Vallée de Bénédiction *עַמְקֵי בְּרָכָה* des Chroniques,† mais le site en est incertain.

 *Pa haqra n - Azai* (Nos. 77-78) renferme le *n* de liaison que nous avons déjà eu dans *Shabbalout ni-gabri*; le déterminatif de la flamme  tient au sens du mot égyptien et non à celui du mot sémitique :

 *ázai*, se rattache soit à *עַז* *capra*, soit à *עוּז*, *robur, potentia, fortitudo*. Site inconnu.  (No. 79) *Adidima* selon la copie de Brugsch,‡ peut se rattacher à la racine *עָדָה* *inluit ornatum, ornavit se* : ce serait alors une forme analogue à celle de *אָדִיתַיִם* *Aditaim* (duplex ornatus) qui désigne une ville de Juda. Je n'en vois pas le site.


 *Zupaqa* (No. 80) est la transcription qu'on admet depuis que Brugsch l'a proposée, bien qu'elle ne présente aucun rapport avec aucune racine connue de l'hébreu. Peut-être serait-il permis de diviser le mot en deux : la première partie serait un dérivé de la racine *נִפְּחָה* *speculatus est, prospectavit*, et la seconde rendrait *בְּיָא* *vallis*, mais tout cela est trop incertain pour que je m'y arrête. Les deux noms qui suivent sont mutilés sans ressource, mais le No. 83

 *Ganat* nous rend un terme connu *גַּנָּת* *hortus*; c'est peut-être le Kharbét Jenneta *خربة جنتا*, qui est situé à quelque distance au Sud-Ouest d'el-Daouaïmèh. En ce cas les localités intermédiaires entre Shoubbalout ni-Gabri et Ganat devraient être cherchées, partie dans la vallée de


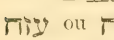

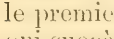
\* Voir plus haut, p. 20.

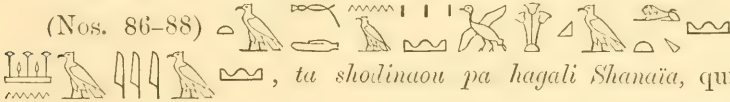

† 2 Chroniques xx, 26, *cf.* dans Josèphe, *Arch.*, ix, 1, 3.

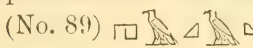

‡ Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 79.

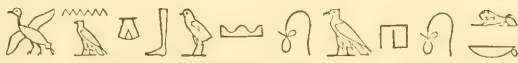



l'Oued en-Nâs, partie dans celle de l'Oued el-Djezair, du Sud au Nord.  (Nos. 84-85)

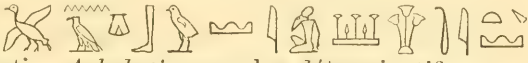


*pa nagabou âzahout* nous fournit un nouvel exemple d'un nom que le scribe a défiguré, en lui donnant une orthographe qui le rapproche d'un mot égyptien : il a décomposé *Azahout* en deux termes dont le premier est devenu pour lui le verbe





 *piller, voler*. Comme il n'y a point de racine  ou , je suis tenté de voir ici encore un composé dont le premier membre serait  *robur, fortitudo*. Les noms qui succèdent à celui-là ne se prêtent pas plus que lui à une identification raisonnable, et il me suffira de les énumérer.

(Nos. 86-88) , *tu shodinaou pa hagali Shanaïa*, qui doit se traduire *les canaux et l'enceinte de Shanaïa* : Shanaïa est probablement un dérivé de la racine  *quievit*.

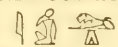


(No. 89)  *Haqa* se rattache à  *murmuravit, susurravit*.

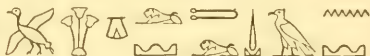
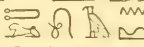

(Nos. 90-91)  *Pa nagabou oucha-ouarouk*, la lande d'Ouhaouarouk.  *Ouarouk* est ici une forme dialectale pour *Barouk*, ainsi qu'on l'a vu plus haut : le  qui précède pourrait être l'article  *oua*.


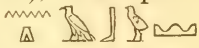
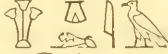

(Nos. 92-93)  la lande d'Ashakati. *Ashakati*, avec les déterminatifs , est un dérivé de  *fovea*.



(Nos. 94-95)  l'Encinte de Haninia, n'est certainement pas le Beït-Hanîna des environs de Jérusalem :\* c'est une localité de Siméon ou des cantons méridionaux de Juda. Haninia dérive d'une forme de  et paraît répondre à . Le nom revient au numéro 99  *Hananii* : il y avait donc, non loin l'une de l'autre, une (*Hagari*) encinte de Haninia et une ville du même nom, comme nous aurons

\* Brugsch, *Geogr. Ins.*, T. II, p. 69.

plus loin les Encintes d'Arad et la ville d'Arad (Nos. 108-111). Entre ces deux endroits ou à côté d'eux on rencontrait (No. 96)  *Alagad, Alougad*, et (No. 97)  *Adimaïm*. Le premier nom renferme peut-être גַּד, גָּדַד, *incidit, irrupit*: אֶל־גַּד; le second se rattache à אָדָם, *ruber, rufus*. Tous ces endroits étaient assez près de Doura, car le No. 100 est  Adora le Grand, qui est identique à Adoraïm et à Dourah.\*

(Nos. 101-102)  , l'Encinte de Toul-Zan; Brugsch lit  *Toul-Ouan*, qui, avec le  *oua* pour ב, équivaldrait à Toul-Ban, Ban étant peut-être בֶּן *filius*.† Le signe douteux a entièrement disparu aujourd'hui, comme j'ai eu l'occasion de le constater à Karnak: il n'est donc plus possible de savoir quelle était la véritable lecture.







(No. 103)  *Haideba* vient de la racine חֵצַב *cecidit lignum*, qui sous la forme חֵטַב signifie *cecidit, excidit lapides*: l'orthographe égyptienne suppose une variante חֶרַב que l'hébreu n'a pas conservée. *Haideba* חֶרְבָּה pourrait signifier un endroit où on taille la pierre, une carrière; comme ce mot revient plus bas (No. 105), on doit peut-être y reconnaître un nom commun comme  *nagabou*,  *Hagara*, qu'il faut réunir au cartouche suivant.  *sic*

(Nos. 103-104) *Haideba Sharounram* et  (Nos. 105-106) *Haideba Diouati* seraient alors les carrières de Sharounram et de Diouati. Le premier nom Sharounram semble de prime abord être étranger aux langues sémitiques; mais la combinaison 

\* Maspero, *Sur les noms de la liste de Thoutmos III qu'on peut rapporter à la Judée*, dans le *Victoria Institute*, T. XII, p. 63-65.

† Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 102.




nr de l'Égyptien n'a, au moins dans les transcriptions de noms étrangers, d'autre valeur que celle d'une *r* ou d'une *l* prononcée fortement. *Sharounram* est donc un dérivé soit de la racine שָׁלַל *traxit, extraxit, spoliavit*, soit de la racine שָׁרַר *torsit, firmus, durus fuit, oppressit*. La terminaison  pourrait répondre à ים du pluriel; mais cette flexion est écrite presque toujours dans notre liste avec une voyelle finale  *má*, et sans . Je pense plutôt qu'il y a ici une faute de graveur et qu'on doit lire  au lieu de  on aurait alors à faire à un nom שררה, שללה. 


*Diouati*, dérive de la racine דָּוַד *amarit*, d'où le nom du roi David, ou de la racine דָּוָה *languit, vgotus fuit*. Aucune de ces localités n'a laissé de traces reconnaissables aujourd'hui.












On s'étonnera peut-être de me voir indiquer avec autant de minutie les racines hébraïques auxquelles il me semble que répondent le nom de notre liste. Ce n'est point affectation de recherche philologique: c'est, je crois, une précaution indispensable dans le genre hasardeux d'étude auquel j'ai été obligé de me livrer. On n'est que trop porté à supposer un erreur de copiste, une faute du scribe qui a compilé la liste, et à intervertir l'ordre des lettres pour obtenir un rapprochement avec un nom connu ancien ou moderne. En montrant que les lettres égyptiennes transcrites en lettres hébraïques nous donnent des formes régulières ou possibles en hébreu, je m'évite à moi-même, et j'évite peut-être aux savants qui traiteront après moi ce sujet, la tentation d'attribuer à une erreur de scribe la présence de tant de noms inconnus, et la faute de modifier ces noms par interversion ou par substitution d'une articulation à une autre articulation. Si nos transcriptions en lettres hébraïques nous donnent des mots réguliers, c'est que les scribes égyptiens ont reproduit aussi exactement que leur alphabet le leur permettait les sons qu'ils entendaient en Judée: nous n'avons donc pas le droit de rien changer à leur transcription.








Les Nos. 108-110 nous fournissent la première identification absolument certaine que nous ayons dans cette partie



de la liste:  *Haqaraim*

*Arada*, les deux enceintes d'Arad et 

 *Rabbit Aradai* רַבֵּת עֶרְד. Les deux enceintes

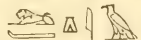
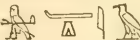
d'Arad étaient dans le voisinage de la ville d'Arad, et peut-être une exploration du pays plus complète nous en ferait-elle aujourd'hui encore découvrir les traces. J'ai à peine besoin de rappeler\* que Rabbat Arad est  Arad de la Bible aujourd'hui Tell Arad . (No. 112)  Iourahma me paraît être identique au  Ierahméel, de la tribu de Juda, dont le Négeb fut pillé par David.  manque à la fin du nom égyptien, mais il manque aussi à la fin du nom arabe Ouady Rahaiméh  واد رحيمه qui a succédé au nom hébreu: la chute du nom divin, qui a permis aux Arabes de transcrire comme ils l'ont fait, est sans doute un fait primitif, puisqu'on trouve dans la liste de Sheshonq  Iourahma. Le No. 111  Nebatout, placé entre Rabbat-Arad et Iourahma, doit être cherché entre Tell Arad et l'Oued-Rahaiméh, mais aucun des noms connus aujourd'hui ne lui ressemble assez pour qu'on ait lieu de l'identifier. Nabatout paraît être un pluriel  נַבְטוֹת d'un mot féminin  נַבְטָה, qui serait dérivé de la racine  נָכַט *conspexit, vidit*.

Trois cartouches ont disparu entièrement derrière Nebatout. Le No. 116  Ari. m., est trop mutilé pour rien donner. Le No. 117  Adorashirau, Adora le petit, par apposition à l'Adora le Grand du No. 86 nous ramène au nord de Ierahméel et d'Arad, probablement dans la partie de la montagne de Juda qui s'étend au Sud d'Hébron, mais sans que j'aie la prétention d'en déterminer exactement le site. Le No. 118, lu par Brugsch  Pabia,† mais dont le premier signe est laissé indécis par Champollion, m'a paru être plutôt  Zabia: rien n'est plus facile en effet que de confondre un  endommagé avec un . Cette lecture est d'autant plus vraisemblable qu'il n'y a plus de racine  פַּב, mais plusieurs



\* Brugsch, qui a le premier reconnu Arad, en a séparé le cartouche  qu'il lit *Lebat*, et identifie à  בֵּית לְבָאוֹת Bêt-Lebaath de Siméon (Brugsch, *Geogr. Ins.*, T. II, p. 69-70).


† Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 118.



racines **צָבָא** *prodiit, processit miles in bellum*, d'où **צָבָא** *exercitus*, **צָבָה** *prodiit stella*, etc. : *Zabia* serait la transcription exacte du nom **צָבִיָה** *doreas femina*, donné aux femmes **צָבִיָה**, **צָבִיָה**, et qui serait appliquée ici à une localité. Le


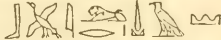
No. 119 est lu par Champollion  *Rahouga*, *Lahouga*, par Brugsch  *Mahouga*. Comme



les racines **רָחַק** *longe discessit*, **לָחַד** *linxit*, **מָחַק** *percussit*, sont là pour appuyer celle des lectures qu'on préférera, l'étude seule de la muraille permettrait de décider entre Champollion et Brugsch. Elle est tellement endommagée aujourd'hui que je n'y ai pu rien reconnaître : peut-être un autre sera-t-il plus heureux. Le No. 120 est évidemment

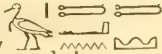
 *Ouariouk* avec  *oua* pour **ב**, comme plus haut, soit un nouveau nom dérivé de la racine **בָּרַךְ**, peut-être le *Kharbét Barouk* **בָּרוּךְ**, de Guérin.

(No. 121)  *Fir-timâa*, renferme, comme

plus haut  *Fit*,\* une forme dialectale *Fir* pour **בְּאֵר**, **בֵּיר** *le puits* : le nom serait **בְּאֵר דִּמְעָה** *Ber-Dimeah*, le puits de la larme, **בְּ** ayant souvent en Égyptien la valeur du **ד** vocalisée en *i* et  équivalant à *bme*. Les noms commençant par *Bir* sont fréquents aujourd'hui dans la région déserte qui s'étend entre Hébron et la Mer Morte, mais aucun d'eux ne rappelle notre *Ber-Diméah*. (No. 122)

 *Abilou* est une **אָבִיל** inconnue, situé dans cette région du désert de Juda. (No. 123) 

*Biar-Rouza*, *Biar-Louza*, est **בְּאֵר לִיזוּ** *Ber-Louza*, *Ber-Louzah*, le puits de l'amandier avec l'orthographe ordinaire par **B**  et non plus par **V**, **F** . Ce *puits de l'amandier* n'a rien de commun avec les deux *Louza* de la Bible, dont l'une était le *Bethel* de Benjamin, et dont l'autre appartenait aux Hittites de la Palestine Septentrionale. Le




nom suivant est  *Bit-Anati*, que j'identifierai comme Brugsch † avec la **בֵּית עֲנֹת** *Bethanoth*




\* Guérin, *Judée*, T. III, p. 164.


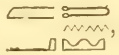
† Brugsch, *Geogr. Ins.*, T. II, p. 70.

de Juda, aujourd'hui Bêt Anoun *خربة بيت عدون*. Ber-Louzah devait se trouver quelque part au S.E. de ce site, sur le versant des montagnes qui descend à la Mer Morte.

Bît-Anat est le dernier nom de la liste qu'on puisse placer sur la carte de manière certaine. Au-delà, il ne subsiste plus que deux cartouches intacts, et les débris plus ou moins lisibles d'une demi-douzaine de cartouches. (No. 125)

 *Shalhatou*, se rattache à la racine *שלה*, *misit*, d'où vient le nom *שלה*, *שלה* de la piscine de Siloéh. Le groupe  qui termine le mot est le pluriel *toou* du mot  *to*, terre, et ne peut guères être employé ici, dans la transcription d'un nom étranger que pour rendre la syllabe *tou*, *toou* :

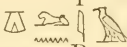
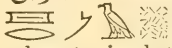
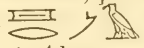
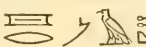
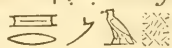



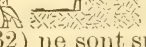
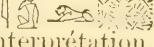
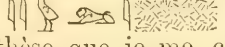
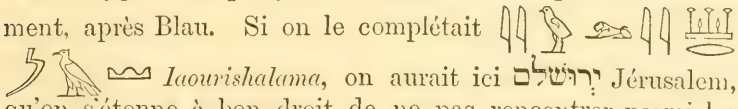
comme  *Shalaha* nous fournit déjà les trois lettres de la racine *שלה*, la finale *tou* est une flexion grammaticale et l'équivalent du   $\text{TI} = \text{T}$ , qui sert à écrire la termination du pluriel féminin. *שלהות* *propagines*, répond exactement à *Shalahaoutou*. Le Livre de Josué nous fait connaître une ville de nom analogue *שלהים* (*armati*) situé dans la partie méridionale de Juda.\* Le site moderne de *Shalahatou* ne m'est pas connu. (No. 126) 

*Alomâten* ou *Armâten* est un nom composé de  *אל* et de , qui peut répondre à *מִדְדִין* *Middin*, ou à toute autre forme de la racine *דדן* *regere, moderari*; j'ai cité *מִדְדִין* de préférence parce que c'est le nom d'une ville de la tribu de Juda.† Comme *Middin* était dans le voisinage de la Mer Morte, c'est-à-dire dans la région où nous savons que les derniers cartouches de notre liste sont situés, on peut se demander si elle n'est pas identique à notre *Alamaten*: *Middin* serait alors l'abréviation d'une forme plus complète *El-Middin*. On ne connaît pas d'ailleurs l'emplacement de *Middin*: seul, Sauley‡ le fixe à *قصر مردة* *Qasr Mirdéh*, le *حربة مرد* *Kharbét Mird* de la carte anglaise, *Mird* *مرد* de

\* Josué xv, 32.

† Josué xv, 61.

‡ F. de Sauley, *Dictionnaire topographique de la Terre-Sainte*, p. 223, s.v. *Meddin*.

Robinson,\* ce qui conviendrait assez à la place qu'Alamâten occupe dans notre liste. (No. 127)  Galouna, Garouna n'est certainement pas, comme Brugsch le pense † la גֹּלָן Gólan de Manassé, qui est trop éloignée du pays où la liste nous oblige à nous maintenir. Il me semble que nous avons ici une transcription exacte du mot גֵּרְיָה, גֵּרְיָן, area: je ne trouve sur la carte aucun endroit nommé جرن Djarn ou جرين Djarín, qui serait la forme arabe du nom antique.  (No. 123) Alama . . . , Arama . . . , renfermant les trois lettres d'une racine אֵלֵם, ne peut se compléter que par un suffixe, probablement celui du pluriel, soit le pluriel masculin  Alamam, soit le pluriel féminin  Amat. Il m'est impossible de dire celui qu'on doit préférer ici, car אֵלְמָה merges, manipulus, frugum, qui est le prototype de  Alama, a les deux pluriels אֵלְמִים et אֵלְמוֹת. Le nom suivant que Brugsch donne ainsi  . . . lahat ou . . . rahat (No. 129), paraît avoir été , soit אֶרְחֹת, pluriel de אֶרַח, via, semita: je ne sais où cette localité était située. Les numéros  . . . la (No. 130)  Mâ . . . (No. 131),  El . . . (No. 132) ne sont susceptibles d'aucune interprétation. Le dernier  (No. 133) Iaoura . . . prête à une hypothèse que je me contente d'indiquer sommairement, après Blau. Si on le complétait  Iaourishalama, on aurait ici יְרוּשָׁלַם Jérusalem, qu'on s'étonne à bon droit de ne pas rencontrer parmi les villes prises par Sheshonq.

Tel est le résultat de cette étude. Les lacunes dont la liste est criblée ne me permettent pas d'affirmer d'une manière certaine que les villes importantes de Juda ou de la Philistie dont les noms n'ont pas été mentionnés, Ascalon,

\* Robinson, *Biblical Researches*, T. II, p. 270.

† Brugsch, *Geogr. Inschriften*, T. II, p. 70.

Ashdod, Gath, Gézer, Hébron, Jéricho, etc., n'y figuraient pas effectivement. Les listes de Thoutmos III, qui sont intactes, présentent tant d'omissions de ce genre que je suis tout prêt à croire, pour mon compte, qu'elles manquaient réellement à celle de Sheshonq, et que les cartouches aujourd'hui détruits ne devaient contenir à l'origine pour la plupart que des noms de localités insignifiantes, aussi obscures que celles dont le souvenir nous a été conservé. Sheshonq avait une surface déterminée de muraille à couvrir, et voulait pour cela un nombre déterminé de noms : les bulletins de son armée et les rapports des prisonniers ou des alliés lui fournirent le nombre dont il avait besoin. On dirait que les scribes ont moins eu l'intention d'énumérer les principales villes de leur nouvelle conquête que d'en fixer le contour : les localités qu'ils choisirent forment autour de Jérusalem et du massif de Juda une sorte de cercle, qui semble suivre assez exactement la frontière du royaume. Beaucoup d'entre elles sont complètement inconnues, d'autres ne sont identifiées que sous toutes réserves avec des noms hébraïques ou arabes, le plus petit nombre est placé sur la carte de façon indubitable. Mon travail n'est ici que provisoire ; j'espère le reprendre plus tard, ou, si je ne puis, d'autres le reprendront et le pousseront plus avant.









SCALE OF ENGLISH MILES  
0 10 20

35° Longitude East of Greenwich.

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Shelford's Geogr. Ed. 1871

MAP TO ACCOMPANY A PAPER, ON THE LIST OF SHESHON, PREPARED BY M. MASPERO FOR THE VICTORIA INSTITUTE.



The following paper was read by Mr. T. G. Pinches, of the Oriental Department of the British Museum, the author being unavoidably absent.

*THE LIST OF SHESHONQ AT KARNAK.*

BY G. MASPERO.

*Translated by Henry George Tomkins.*

**T**HE list which Sheshonq I, caused to be sculptured of the towns that he had taken, or professed to have taken, during his campaign against Rehoboam, has often been studied by Egyptologists. Champollion the younger\* and Osburn† extracted all that was possible at a period when neither the structure of hieroglyphic writing nor the geography of Palestine was well known. Brugsch has since given a complete analysis‡ which has enabled Blau to propose a more serious explanation than any which had hitherto been made.§ The commentary of Blau, modified by

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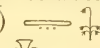






\* Champollion, *Lettres écrites d'Égypte*, p. 99-100, *Grammaire Égyptienne*, p. 160, and *Monuments, Texte*, T. II, p. 114.

† Osburn, *Egypt, her Testimony to the Truth*, p. 158-162.

‡ Brugsch, *Geogr. Ins.*, T. II, p. 114.

§ Blau, *Sisags Zug gegen Juda aus dem Denkmale bei Karnak erläutert*, in the *Zeitschrift der deutschen Morgenländischen Gesellschaft*, T. XV, p. 233 sqq.

Brugsch in the second edition of his "History of Egypt,"\* has since remained almost classic in the science, and most of the identifications which he had reached have been admitted without discussion by archaeologists and geographers.† A first examination, made in 1880, shewed me, however, that Blau had taken too great liberties with the outward form of the names, and had obtained many of his approximations only at the price of inversions and alterations too numerous to be permissible: I have since endeavoured to prove that the Egyptian letters, transcribed rigorously in Hebrew letters, give almost everywhere the regular Hebrew forms, and therefore need neither changes nor corrections.‡ I desire, in the present paper, to collect, after nearly ten years of fresh research, the partial results at which I have arrived, and to submit them, with due reserve, to the criticism of my brethren in Egyptology. I have ascertained the text by comparison of all the copies published since the beginning of the century, and by collation of Champollion's copy with what still remains of the original on the wall at Karnak.§

The first ten cartouches were filled by the general forms which we meet with at the starting of most geographical lists. Although a certain number of them are quite destroyed, we may restore them with certainty: (No. 1)  *To-qimâiti*, the country of the south, (No. 2)  *To-mihiti*, the country of the North, (No. 3)  the tribes situated between the Nile and the Red Sea, from the latitude of Assouan to that of Siout, (No. 4)  the *Tahonou*, the Berber tribes beyond the Oasis of the Thebaid, corresponding to the *Aniou* of the last cartouche, (No. 5)  the Bédouins who live between the Nile and the Red Sea, from the latitude of Siout to the neighbourhood of the Ouady Toumilât, (No. 6)  the Berber tribes who occupy the Oases to the west of the Birket Kéroun, parallel to the Pittiou Bédouins, (No. 7)  the *Montiou*, the

\* Brugsch, *Geschichte Ägyptens*, p. 660-663.

† It is from Blau's Memoir that Mariette has borrowed that strange hypothesis of Egyptian army-corps manœuvring as modern army-corps do (*Les Listes des Pylones de Karnak*, p. 46-48).

‡ Maspero, *Notes sur différents Points de Grammaire et d'Histoire*, in the *Zeitschrift*, 1880, p. 44-49.

§ Maspero, *Révision des Listes géographiques de Thoutmos III*, p. 100-101.


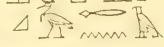
Bédouins of Arabia Petraea between Egypt and Syria, (No. 8) the *Palition*, nomads of Syria between the Montiou and the frontiers of Naharamma,\* lastly the (No. 9) *Haïounivou* of Asia Minor and the Isles of the Mediterranean. The last cartouche (No. 10) is the only one which I cannot recollect to have met with elsewhere. Perhaps it should be restored *copy of the Asiatics*, considering these words as a sort of title applying to the whole of the following names: this is, however, a mere conjecture. The thirteen cartouches which come next present no greater difficulties of interpretation. I content myself with enumerating them with the restorations which I thought right to add in 1880, and in which I see at present nothing to change. (No. 11) *Gazatou*, Gaza, [No. 12 *Magidi*, Mageddo], (No. 13) *Rabbati*, רבית of Issachar, (No. 14) *Tainaou*, תענא, (No. 15) *Shaunama*, שונים of Issachar, † (No. 16) *Bit-Shaïlla*, where I seem to recognize the Shiloh שילה of Ephraïm, ‡ (No. 17) *Rouhaïba* רחב, now Rehab, § to the south of Béisan, (No. 18) *Hapourama*, חפרים of Issachar, (No. 19) *Adoulmin* אדלמים, which neither the hieroglyphic spelling, nor the position which it occupies in the list, permit us to identify with the Adullam אדולים of Judah. No. 20, which is entirely mutilated, perhaps contained the name of Sichem, one of the capitals of Israel under Jeroboam I. No. 21 *Shaouadi*, is doubtless *Souïda*, (Kharbét es-Suweidéh of the English map),

\* On these peoples see J. de Rougé, *Textes géographiques du Temple d'Edfou (II<sup>e</sup>-Égypte)*, taken from the *Révue Archéologique*, 1865, p. 12-16.



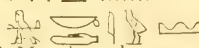
† The identification was proposed for the first time by Osburn, *Egypt*, p. 158.



‡ *Sur les Noms géographiques de la Liste de Thoutmôs III qu'on peut rapporter à la Judée*, in the *Transactions of the Victoria Institute*, Vol. XXII, p. 69-70.

§ *Cfr.* Eusebius, *Onomasticon*: καὶ ἐστὶ Ῥωὺβ κόμη ἀπὸ σημείου Σκυθοπόλεως, ἣν δὲ Δευίταις ἀφωρισμένη (edit. Parthey, p. 316).

a little way from the right bank of the Jordan.\* The following numbers represent  Mahanaïm מַחְנַיִם beyond Jordan,† and  Gibeon גִּבְעוֹן of Benjamin.‡ The presence on the list of towns belonging to Jeroboam does not prove that the Egyptian armies had penetrated into Galilee or passed the Jordan. The king of Israel, in imploring the aid of Sheshouq against his rival, had thereby made himself vassal to Egypt: this would suffice to make his towns figure at Karnak among the cities subjected in the course of the campaign.

To determine the site of the places which occur beyond Gabaon, I have used the process which has already availed me with regard to the lists of Thoutmos III: I have divided the list in sections, each comprised between two towns already known, and whose position in the land has been indicated, if not surely at least probably, by recent explorers. From Gabaon to the vacant place of No. 30, the Egyptian scribe has followed the line of places or fortified posts which covered the northern frontier of the kingdom of Judah.

(No. 24)  *Bit-haouaroun* is the Beth-horon בֵּית-הַחֲרוֹן of Ephraim.§ (No. 26)  *Aiaouloun*, the Aialon אֵיָלוֹן of Dan.|| (No. 27)  *Makidau*. מִקְדָּה, ¶ that is Bêt-Our, Yalo, and Magharah. مغارة

\* The determinative  is still very visible in the original, and completes the word (Maspero, *Recueil*, T. VII, p. 100). Blau (*op. l.*, p. 237) restores  which he identifies with عبرق السودان

Arak es-Soudan, or السويدان es-Souêidan of Robinson (*Palestine*, III, p. 867, II, p. 657).

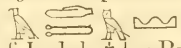

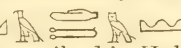
† Champollion, *Grammaire Égyptienne*, p. 160, *Monuments, Texte*, T. II, p. 114; Rosellini, *Mon. St.*, T. IV, p. 157.

‡ Brugsch, *Geogr. Ins.*, T. II, p. 61.

§ Champollion, *Grammaire*, p. 160, *Monuments, Texte*, T. II, p. 114; Rosellini, *Mon. Stor.*, T. IV, p. 157.

|| Champollion, *Monuments, Texte*, T. II, p. 114; Brugsch, *G. Ins.*, T. II, p. 62.

¶ Champollion (*Mon.*, *Texte*, T. II, p. 114, and *Grammaire*, p. 160), Rosellini (*Mon. St.*, T. IV, p. 157-158), Osburn, *Egypt, Her Testimony to the Truth*, p. 160), Brugsch (*G. Ins.*, T. II, p. 62), E. de Rougé (*Mémoire sur l'Origine*, p. 53), have wished to recognize Mageddo here; Blau (*op. l.*, p. 237-238) has remarked that Mageddo would be here out of its place, and has proposed Makkedah, which I have accepted (*Zeitschrift*, 1880, p. 45).

el-Moghâr,\* some distance S.E. of Yebnah. This group of ascertained positions permits us to reject, *à priori*, the identifications proposed for (No. 25)  *Qadoutim*, by Champollion, with עֵיטָם Etham of Judah,† by Brugsch and Rougé with קְדֵמוֹת Kedemôth of Reuben.‡ Blau thinks of the town Adithaïm עֲדִיתַיִם, which should be somewhere in the neighbourhood, and I have formerly believed that I could accept this identification: the transcription  $\triangle$  Q for ע may in fact be justified by the example of  *Qazatou*, Gaza, עֲזָה.§ It should always be noticed that in names where the Hebrew ע tends to the pronunciation of Arabic ع, the Greek versions and Vulgate have commonly a γ, g, as in Γάζα, Γαζ, Ἀγγαζ = עֵי, חָעֵי, Γαδὲρ = בְּגִדְלֵעָר, Βεθφοργόρ = בֵּית־פְּעוֹר, &c.; now עֲדִיתַיִם is never transcribed Γαδιθαΐμ, but Ἀδιθαΐμ, which seems to show that in this word the initial letter is ע pure without the tendency towards ع, and this leads me to reject the equivalence of עֲדִיתַיִם Adithaïm and  *Qadoutim*. Besides, the hieroglyphic group transcribed in Hebrew letters gives us a form גְּרֹודִים from גְּרֹוד, *incisio, sulcus, turma, agmen militum*, which obliges us to consider it as having been correctly marked by the scribe and by the sculptors of Karnak. The modern equivalent of this name would be, with omission of the plural termination ים and substitution of ג for ח, a word *Djédid* or *Djédoud*, which would infallibly confuse itself with the adjective جديد, *new*: the *Djoudéidichs* which we meet with in many places in our maps are too far from Aïalon and from Bethhoron to enable us to compare them with our *Qadoutim* or *Qadoudim*. This place should be found between Bêt-our and Yalo, probably towards the point where the road joining these two towns clears the Wady

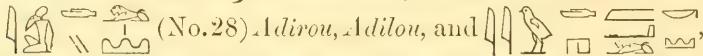



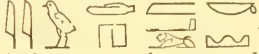
\* The site of el-Moghâr, proposed for Makkedah by MM. Warren (*Palestine Exploration Fund Quart. Stat.*, 1875, p. 181) and Conder (*Quart. St.*, 1875, p. 165-167), is the most probable of all those that have been thought of at present.

† Champollion, *Mon., Texte*, T. II, p. 114.

‡ Brugsch, *Geogr. Ins.*, T. II, p. 61; E. de Rougé, *Mémoire sur l'Origine*, p. 53, 91.

§ Blau, *Sisaqs Zug*, in the *Z.d.d.M.*, XV, p. 237; Maspero, in the *Zeitschrift*, 1880, p. 45.

Sulimân, and about the place where it crosses the way to Jerusalem; the English map shews in this place a Kharbèt Bêt-Nashef خربة بيت ناشف, which may be *Qadoutim*, *Qadoudim*.

 (No. 28) *Adirou*, *Adilou*, and , *Taoudhamalouk* (No. 29), have been the object of various and contradictory hypotheses. As the direction in which we may meet with *Adirou* depends on that in which we may find *Iaoudhamalouk*, I have already troubled myself about this latter name. Champollion has seen in it the symbol of the kingdom of Judah,\* Rosellini that of the king of Judah.† E. de Rougé persisted to the end in holding the opinion of the first Egyptologists as probable, if not certain.‡ Brugsch, on the other hand, thought that we have here a mere village whose modern representative would be a *Yahoudiyéh*, *el-Yahoudiyéh*, either that in the neighbourhood of Tibnîn, or that in the neighbourhood of Jaffa.§ Blau held to the opinion of Brugsch, and added that our locality, being situated in the neighbourhood of *Makkédah*, must be identical with the יהוד *Iehoud* of Dan, that is to say with *el-Yehoudiéh* near Jaffa:¶ *Iaoudhamalouk* would be the full form; *Iehoud* an abridged form. Of late M. Max Müller has undertaken to show that the Egyptian  does not contain the name of Judah: we ought, says he, to find in the Egyptian the first ה of יהודה and to have  in place of . He thinks that the initial element of the name is יד, "the hand," and that the whole corresponds to יד־הַמֶּלֶךְ *Id-hammelek*: the town *Idhammelek*

\* Champollion, *Lettres écrites d'Égypte*, p. 90.











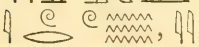



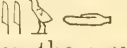



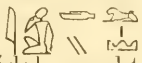


† Rosellini, after having adopted the opinion of Champollion (*Mon. Stor.*, T. II, p. 79-80), was induced by philological scruples to modify it, and to see in the name of our locality the mention of the king of Judah (*Mon. Stor.*, T. IV, p. 158-159).

‡ E. de Rougé, *Mémoire sur l'Origine égyptienne de l'Alphabet phénicien*, p. 53, *Leçons de M. de Rougé* in the *Mélanges*, T. II, p. 274, note 9. M. de Rougé, or M. Robiou, who has published the *Leçons*, attributes to Champollion the translation of Rosellini and returns to the sense of *kingdom*, which is precisely that which Champollion had proposed.

§ Brugsch, *Geogr. Ins.*, T. II, p. 62-63.

¶ Blau, *Sisaks Zug*, in the *Z.d.d.M.*, T. XV, p. 238.








would have no longer anything in common with Iehoud of Dan.\* In spite of the very high esteem which I have for Herr Max Müller (of Nürnberg), and although Mr. Le Page Renouf has lent him the support of his authority, I do not think the objection he has raised against the reading of Brugsch is insurmountable. The word  contains a troublesome element, the *ou*  which follows the two feathers , and which has no business there if we allow the reading ; so MM. Müller and le Page Renouf have tried to get quit of it by explaining that we need not take it into account. I think on the contrary that we must never neglect it, neither in this case nor in other cases where it occurs. The sign  in the beginning of a word appears to have always an *a* for its inherent vowel, and this *a* with diphthongal *ou*, which is sometimes expressed, sometimes suppressed, in writing, has given in Coptic sometimes  $\alpha$  or  $\epsilon$ , sometimes  $o$ ,  $\omega$ : , , *iaoumé*, , the sea, , *B.*, , *T.M.*, , , *iaourou*, the river, , *T.*, , *M.*, &c. I read then  *Iaoud*, not *Ioud*, and I think that the hiatus between the *a* and *ou* renders the very weak aspirate of  between two vowels, in the same manner that it does in the transcription *Iaoudaï* in Assyrian texts.  *Iaoud* will thus form the transition between the full form of the classic Hebrew  and the reduced form *Ioudás* of the Greeks. I will retain, till further intelligence, the reading *Iaoud-ha-malouk*, *Iehoud-ham-melek*, and the identification with *Iehoud* of Dan, *el-Yâhoudiyéh*. No. 28  *Adirou*, placed between this locality and *Makkédah*, ought then to have its place marked in the plain between *el-Yâhoudiyéh* and *Môghar*. Blau professes to recognize in it a bad Egyptian version of , for, says he, a town so important as *Lydda* would not be missing in the list of Sheshonq.† Brugsch has very justly remarked that 


\* Max Müller, *The supposed name of Judah in the list of Sheshonq*, in the *Proceedings of the Society of Biblical Archaeology*, Vol. X, p. 81-83; after this memoir we find (p. 83-86) some *Remarks* in which M. Le Page Renouf approves the conclusions of M. Max Müller.


† Blau, *Sisaks Zug*, in the *Z.d.d.M.*, T. XV, p. 221.

renders letter for letter a derivative of the root **אָדַר**, *amplus fuit*, doubtless **אָדַיר**, *amplus, potens*.\* This name *Addirou*, *Addir*, presents so close a consonance with the Arabic **الدير** *al-dêir*, *ed-dêir*, that it may be confounded with one of the numerous *dêirs* which figure on our modern maps. Having regard to the position which it holds in the list, I am tempted to recognize it in the **خربة ديران** *Kharbét-Dêirân* which the English map shews at some distance north of el-Môghâr.

The cartouche No. 30 is destroyed with the exception of the sign , which tells us nothing. The series which begins beyond, and which is broken after No. 40 by a long gap, offers us towards the end a name (No. 38)  *Shaouka*, which is written  *Saouka* in the list of Thoutmos III (No. 69): it is the Shokoh of the Judæan plain, now *Kharbét-Shouwêikéh*.† The names comprised between *Iaoud-ham-melek* and *Shaouka* must then range themselves more or less regularly between el-Yahoudiyéh and *Kharbét-Shouwêikéh*. For the rest, No. 35, which is mutilated , may be restored with certainty in

 *Iaouhama*, which is the (No. 68)

 *Iouhamâ*, *Iaouhamâ*, of the campaigns of Thoutmos III: now *Iaouhama* is, as Sauley has shewn,‡ el-Khéiméh, **الخيمة**. This restoration permits us to divide the series into two sections, one of four names from *Iaoud-ham-melek* to *Iaouhama*, one of five from *Iaouhama* to *Shaouka* and beyond. The four names which compose the former section are unhappily not easy to identify. No. 31

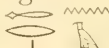
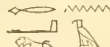

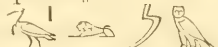
 *Haianim*, *Hianim*, appears as a plural form **חַיִּים** of a word derived from the root **חָנַן**, *propitius fuit*.


*favit alicui*; it is the same in meaning as **בֵּית־חָנָן** of Dan or Judah, and I am disposed to compare it with *Kharbét-Hannounâh* **خربة حنونة**, situated in the hills to the east of

\* Brugsch, *Geogr. Ins.*, T. II, p. 62.


† Brugsch, *Geogr. Ins.*, T. II, p. 64.

‡ F. de Sauley, *Lettre à M. Chabas*, in the *Mélanges d'Archéologie égyptienne*, T. I, p. 122-123.

el-Yahoudiyéh,\* near to which the English map marks some ruins.† The name following (No. 32)  Alana. Alouna. would be transcribed in Hebrew letters עֵלֶן, עֵלֶן, and consequently cannot be either the פֵּלֶן of Dan, or Eglon עֵגְלוֹן, as Brugsch‡ and Blau:§ would have it: we have here a locality bearing the same name as the  of Thoutmos III, where I have recognized a variant of the Hebrew עֵלְיוֹן, superior, suprema, summus. The present nomenclature gives us many عَلَيْن Alins, of which the orthography agrees perfectly with that of our town. The only one whose site can rigorously agree is that which the English map calls Kharbét Ouady Alin خربة وادي علين, and which is to the north of Shouwêikéh, near 'Ain Shems, where the Beth-Shemesh of Judah is generally placed; yet the space will be too great between Kharbét Hammounah and Kharbét Ouady Alin for me to venture any affirmation on it. Brugsch reads (No. 33)  Biloma, and identifies it with בִּלְעָם Bileam of Manasséh, in spite of the absence of ע in the Egyptian transcription.|| In fact Champollion, Rosellini, and Lepsius all give  Biroumam, Biloumam, and this is the reading which can still be deciphered on the wall. Biroumim or Biloumim is in fact the regular plural of a word derived either from the root בָּלַם *constrinxit, clausit*, or from the root בָּרַם *frenuit*: בְּרוּמִים, which denotes the name of a sort of embroidered fabric, is here reproduced letter for letter. Unhappily I cannot find on any map any modern name which recalls that of Biroumim or Biloumim.

 (No. 34) Zaidipoutir or Zaidipoudil is known

\* Guérin, *Samarie*, T. II, p. 74.



† Osburn (*Egypt, her Testimony*, p. 160) has compared Haianim with הַיְהוֹנִים, the valley of Hinnom. Without speaking of the topographical difficulties which this identification presents,  never answers to הַיְהוֹנִים in Hebrew.


‡ Brugsch, *Geogr. Ins.*, T. II, p. 63.

§ Blau, *Sisaks Zug*, in the *Z.d.d.M.*, T. XV, p. 235.



|| Brugsch, *Geogr. Ins.*, T. II, p. 64.

to us in *Papyrus Anastasi* No. 1, under the form 

□  *Zidipouti* or *Zaidipouti*, with loss of  R, L, final.\*

The name is evidently formed of two words. The first belongs to the root צוּר, *venari*, and, after the vocalization of the Egyptian in , answers more particularly to צוּר *venatio*, *fera venando, capta, cibus, comneatus*: it is the same origin as that of the Sidon צוּר of the Phœnicians. The second name

may be compared with different roots, פָּרָר, whence פָּרָר *adepts*, פָּמַר *fidit*, פָּתַל *contorsit, nevit*, פָּתַר *interpretatus est somnium*, among which I leave to the reader the task of choosing that which pleases him best. These compound names get shortened in any fashion: if we admit that the second element


 *poutir* has lapsed, the first  *Zaidi*,

immediately recalls the town of صيدون Saïdoum, Sïdoum, which is situated a little south of Tell-Djezer, and north of Kheïmèh. Guérin here notices some ruins which make him suppose in this place the existence "of an ancient village which history does not mention, and which, in contrast to the great town of Sidon, its namesake, has always remained obscure, and probably without importance."† The position of Saïdoum fits the position which Zaïdi-poutir occupies near Iaouhamâ.

Brugsch read for No. 36  *Bît-âlemat*,

and proposed to recognize in it עֲלָמֵת *Allemet*, עֲלָמוֹן *Almôn* of Benjamin.‡ now 'Almît to the N.E. of Jerusalem. The reading and identification have been accepted by Rougé.§

Blau for his part prefers to transcribe בֵּית-עֲלָם *Bêth-Olam*,|| and declares for the Beit-alam بیتِ عالم of Robinson.¶ Since the copies of Champollion, Rosellini and Lepsius bear

 *Bi-âlamim*, I have thought fit to reject

\* *Papyrus Anastasi I*, pl. xxii, l. 5; Chabas, *Voyage d'un Égyptien*, p. 199, has been the first to think of comparing the list of Sheshonq with that of the Anastasi Papyrus. He does not propose any identification.

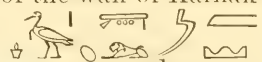
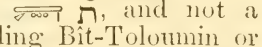

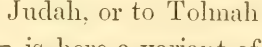
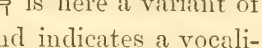
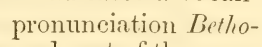
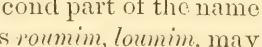
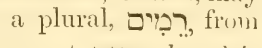
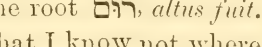
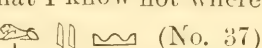
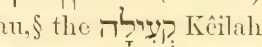
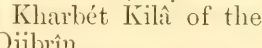
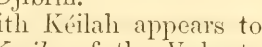
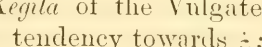
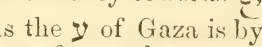
† Guérin, *La Judée*, T. II, p. 33.

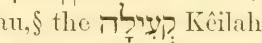
‡ Brugsch, *Geogr. Ins.*, T. II, p. 64

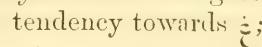
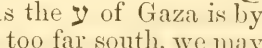
§ E. de Rougé, *Mémoire sur l'Origine de l'Alphabet phénicien*, p. 95.

|| Blau, *Sisags Zug*, in the *Z.d.d.M.*, T. XV, p. 238.

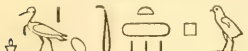
¶ Robinson, *Biblical Researches in Palestine*, Vol. II, p. 403; *cf.* Guérin, *Judée*, T. II, p. 369.

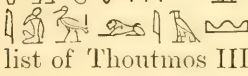
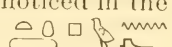
the two hypotheses.\* An examination of the wall of Karnak has shown me that we ought to write  *Bito-Loumin* or *Bito-Roumin* with a , and not a .† One cannot dream of a reading Bit-Toloumin or Bit-Touroumîm, which will give us a name equivalent either to the Telem  or  *Telaïm* of Judah, or to Tolmah  of modern maps.‡  is here a variant of the final , *ti, t* of  *Bit*, and indicates a vocalization in *o* analogous to that which the pronunciation *Bethogabra, Betogabris*, discloses to us. The second part of the name  *rounam, lounam*, or perhaps *roumin, lounim*, may be a singular word as  *elatio*, or a plural,  *rûmîm*, from  *bubalus*, or from a derivative of the root  *altus fuit*. *Bit-roumin* is a correct name, but one that I know not where to place on the land.  (No. 37)


*Qaqali, Qaqari*, is, in the opinion of Blau,§ the  *Kêilah* of Judah, which we identify with the *Kharbét Kîlâ* of the *Ouady Souwêidéh*, to the east of *Beit-Djibrîn*.


The comparison of *Qaqali, Qaqari* with *Kêilah* appears to me legitimate, for the transcription *Kegila* of the Vulgate shews that the *y* has in this name the tendency towards ; it may be rendered by  $\Delta$  in Egyptian, as the *y* of *Gaza* is by . If we find that the site of *Kêilah* is too far south, we may think of another *Kharbét Kîla* which the English map marks to the west of *Eshouâ*, and whose situation north of *Shouwêikéh* and east of *Khéiméh* is sufficiently in accord with the position of *Qaqali* between *Iaouhamâ* and *Shaouka*.

The two succeeding names after *Shaouka* (No. 39)

 *Bit-tioupon, Bit-Toupon*, and (No. 40)

 *Abila*, have been already noticed in the list of *Thoutmos III* with the spelling (No. 98) 

*Tiouponou, Tipounou*, and (No. 99)  *Aoubilou*, in the same order assigned to them by *Sheshonq's* scribe.



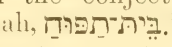


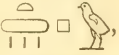









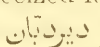

The omission or addition of  *y* is a thing too



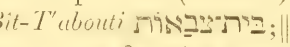
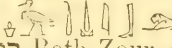
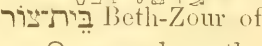

\* Maspero, in the *Zeitschrift*, 1880, p. 46.

† Maspero, in the *Recueil*, T. VII, p. 100.

‡ Guérin, *Judée*, T. I, p. 256.

§ Blau, *Sisazs Zug*, in the *Z.d.d.M.*, T. XV, p. 238.

frequent in Jewish nomenclature to surprise us here, and the final  nou occurs in  Loudni, for example, for Loud. I have already rejected the conjecture of Brugsch, *Bit-Tipounou* = Beth-Tappouah, .\* Blau only admitted it after Brugsch assured him that he had found the equivalent of final  on the stone.† I have ascertained that this  does not exist really, and that the wall bears   , not    or    , and the absence of this letter suffices to render the comparison of the two names impossible. The parallel with *Téphón*, *Topho* of the 1st book of Maccabees IX, 50, is only admissible if this unknown *Téphón* is not a Grecized form of Beth-Tappouakh, Tappouakh.‡ If the name  *Dêir Doubbân*§ may be considered as arising from a caprice of popular etymology, which has substituted the word  for an ancient name *Bit-Toupou*, *Toupounou* may have occupied the site which the fellahs call after that fashion. The Abila of No. 40 has disappeared.

The great gap which we notice after Abila accounts for no less than twelve cartouches, of which two only admit of restoration. Brugsch has already completed (No. 46)  as  *Bit-T'abouti* ;|| Blau afterwards thought of restoring it as  *Bit-T'abiro*, which he affirmed to be  Beth-Zour of Judah.¶ Yet, if we have sometimes a  *oua* where the

\* Maspero, *Sur les Noms de la Liste de Thontmos III qu'on peut rapporter à la Judée*, in the *Transactions of the Victoria Institute*, Vol. XXII, p. 167-68.



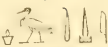

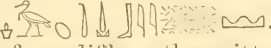
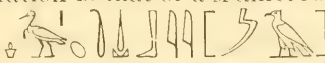
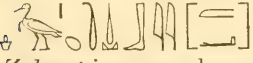
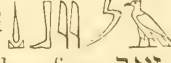








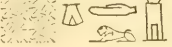
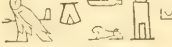
† Blau, *Sisaqs Zug*, in the *Z.d.d.M.*, T. XV, p. 238. "Das schliessende *h'* nach Brugsch's mündlicher Mittheilung wirklich von ihm nachträglich auf dem Steine gefunden worden ist."

‡ *Maccabees* I, ix, 50. Similarly in the name of *Καφαρόφα* which Ruffinus appears to have read in Josephus (*de Bello*, V, 4) in the place of *Καφαρόφα* (Reland, *Palästina*, T. II, p. 692). Neubauer (*Géographie du Talmud*, p. 112) is inclined to recognize here Tappouakh, which hinders me from comparing it with our *Bit-Toupou*.

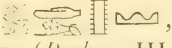
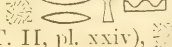


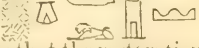
§ Robinson, *Biblical Researches*, T. II, p. 2-354, 421, who is inclined to recognize in this place the site of Gath-Rimmon; *cf.* Guérin *Judée*, T. II, p. 104-6.


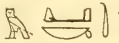
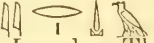
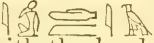
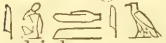

|| Brugsch, *Geogr. Ins.*, T. II, p. 65.

¶ Blau, *Sisaqs Zug*, in the *Z.d.d.M.*, T. XV, p. 238-9. On the authority of Blau, Brugsch would have adopted the identification with Beth-Zour.

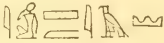

Hebrews puts a ב, we have never a  for a Hebrew ב, and בית צור would be transcribed  not  . The copies of Champollion, Rosellini, and Lepsius differ slightly from that of Brugsch, and I have myself been able to verify the original text as .\* The only possible restoration is that of a M differently written and vocalized, either  or  *Bit-Zabima*. The word  *Zabimâ* is a regular plural which may come either from זאב, *wolf*, בית זאבים *Beth-Zebim, the house of wolves*, or even from צבוע *hyæna*, בית צבועים *Beth-Zeboïm, the house of hyænas*: the tribe of Benjamin had similarly a valley of hyænas, גי הצבועים. No. 47 occurs under the form , where only one long sign is wanting between  and the eagle . By restoring  we should have the equivalent of כוכב, *star*, but this restitution, attractive as it may be, is unlikely; the  is never followed by  in our list, and the final  on the contrary is here so frequent that the reading  *Koka* forces itself on us. The name answers to the Hebrew גג, *גנה, roof, surface of the altar*, and will apply very well to a village situated on the terraced top of a hill. I will not offer any conjecture as to the situation of these two places; the lacuna which contains them is too large to enable us to determine even the direction in which we ought to seek them. The list begins again with No. 52, and continues, not without mutilations, till No. 58. The last two numbers, 57 and 58, have suffered slightly, but are restored with certainty: No. 58  is  *Magdilou*, and

\* Maspero, in the *Recueil*, T. VII, p. 100.

† Champollion (*Mouuments, Texte*, T. II, p. 116) has , Rosellini (*M. St.*, pl. cxlviii), , Lepsius (*Denkm.*, III, bl. 252), Brugsch (*Geogr. Ins.*, T. II, pl. xxiv), . I have restored from all these copies the reading  (*Zeitschrift*, 1880, p. 46); the wall at Karnak has  (Maspero, in the *Recueil*, T. VII, p. 100), which proves that the restoration was exact.

No. 59  Iarza.\* The two names have already figured in the lists of Thoutmos III,  Magdilou (No. 71) and  (No. 69), and are now el-Medjdel and Kharbét Ierzah. There is therefore great likelihood that part at least of the names which immediately precede these two belonged to places near Kharbét Ierzah and el-Medjdel. No. 56  Adima† has been identified by Brugsch with the land of Edom, which Rougé does not deny.‡ The comparison, blameless from a philological point of view, falls of itself when we remember that our lists do not contain the names of extensive countries, but only the designation of features of the land, towns, or villages.  Adouma must then here indicate a town which was called אֲדוּמָה *the red*, or אֲדָמָה *the field*, like one of the villages of the tribe of Naphtali, or like one of the five accursed towns אֲדָמָה. The only modern name which can be set beside Adouma is that of Bêt-Timéh بیت طیمة, a large village situated to the east of Ierzah, where Guérin mentions the existence of Roman or Byzantine ruins:§ the site will very well suit the relative position of names in the list, and the modern orthography may strictly be deduced from the ancient. No. 58  Zaloumim, is a plural of the words צֶלֶם, *shade*, but cannot be identical with the station צֶלְמוֹנָה Zalmonah of the itinerary of the Hebrews in the desert, as Brugsch would have it: || the vicinity of such names as Migdol and Ierzah does not authorize us to go down so far to the south in this part of our list. We find no longer in the country any name which recalls that of Zaloumim, but the word itself is expressive, and may recal the shades of the gardens which surround el-Medjdel: a site

\* The restoration Ierza, which I proposed in 1880 (*Zeitschrift*, 1880, p. 46), has been confirmed by the examination of the original text, (*Recueil*, T. VII, p. 100).


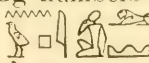
† Champollion, *Monuments, Texte*, T. II, p. 116) read  (cfr. *Zeitschrift*, 1880, p. 46): the wall really has Adima, with a , as Brugsch said (*Geogr. Ins.*, T. II, pl. xxiv).






‡ Brugsch, *Geogr. Ins.*, T. II, p. 66-67; E. de Rougé, *Memoire sur l'Origine de l'Alphabet phénicien*, p. 53.

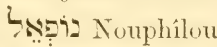
§ Guérin, *Judée*, T. II, p. 127-128.

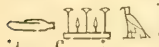
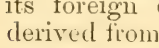
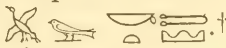
|| Brugsch, *Geogr. Ins.*, T. II, p. 67.





such as that of the village of Hammâméh  would closely suit what we conjecture of Zaloumim, Zelemin. The three preceding numbers ought not to be situated far from el-Medjdel.  Noup-ïlou (No. 53) is com-



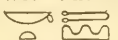
posed of the final , God, and a derivative of the root  *agitavit*, or  *eminentia*,  *locus, editus*. The town of  Neballat, Beit-Nebâla, in the tribe of Benjamin, which



Blau recognizes here, does not answer to  Noup-ïlou either in orthography or in position: \* I do not see elsewhere, between Shouëikéh and el-Medjdel, any name which seems derived from the ancient name and fit to be assimilated to it.


 (No. 54) *Dishati, Doushati*, is, in spite of its foreign cast, a Semitic name. It is a feminine form derived from the root  *terere, conterere, triturare frumentum*; but when this is said, I know no way of locating the town in the land. No. 55 is not only hard to place, but hard to decipher. I read it as Champollion did, .†

It includes the Egyptian masculine article  *pa*, of which we have numerous examples in  *Pa Haqala*,

 *Pa anaqou*,  *pa nayabou*.





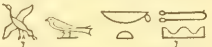
The sign following is most likely  *oïrou*, the Great, the Chief, which occurs almost as often without as with its final  in the hieroglyphic texts.  transcribed in



Hebrew gives us  *גתות*, plural of the word  *גת* a *press*. Osburn has proposed to translate the whole as *The prince of the Gadites*,‡ and in fact no philological reason prevents our translating thus the first two words; only the latter certainly does not designate the people of Gad, who had nothing to do with these parts. It would be very pleasant to translate with confidence *the chief of Gath*, but it does not seem to me

\* Blau, *Sisags Zug*, in the *Z.d.d.M.*, T. XV, p. 240. Brugsch, after having recognized the real etymology (*Geogr. Ins.*, T. II, p. 65), inclined to recognize in Noup-ïlou an equivalent of  Pnuel, which answers neither to the spelling of the name, nor to the place which it occupies in the list of Sheshonq.

† Champollion, *Monuments, Texte*, T. II, p. 116.

‡ Osburn, *Egypt, her Testimony to the Truth*, p. 162.

that *Kitout* can be גת. This interpretation does not get us out of our difficulties, as we see; it has even the inconvenience of raising a very strong objection. The chiefs of conquered lands never figure in the lists: the cartouche No. 54 of Sheshonq would be an unique case in Egyptian archaeology if we ought to take it in Osburn's light. My impression then is that we ought not here to divide the group: it should be considered as forming one single word *Pouroukit*, *Paouroukit*, which the scribe amused himself by writing with elements giving in Egyptian a sense which has nothing in common with the original meaning in Hebrew. He has transcribed the first syllable *Pour* , *paourou*, the chief, in lieu of  for example; he has chosen for the second, *kit*, the term , a graphic variant of  *kiti*, which signifies *other*, and the name *Pouroukit* finds its analysis in an Egyptian phrase more or less correct which would mean *the other chief*.  *Pouroukiti*, *Pouloukiti*, is for me the word פלגות, from פלגה, פלג, *a stream*, in Arabic فَلَاحٌ، فَلَاحٌ. We have in fact in the district of el-Medjdel a town of فالوجا Faloudja,\* el-fالوجى† el-Faloudjy, which for name and position appears to me to agree with our Pouloukiti.

A new lacuna of four cartouches breaks the series afresh. No. 64,  ... *papen*, has been completed by Blau‡ as 'Aapen, עפני Ophni, Gophmah: the restoration has taken no account of the middle syllable *pa*, and is indefensible, but I know no more than Brugsch§ how to complete the group. Happily No. 65 furnishes us a precious indication: Brugsch has recognized that  *Azama*|| answers either to the Azem עים of Judah, afterwards reckoned to Simeon,¶ or to עזמון Azimon, which marks the southern

\* Guérin, *Judée*, T. II, p. 124.

† Robinson, *Biblical Researches in Palestine*, Vol. II, p. 421, Vol. III, *App.*, p. 209.

‡ Blau, *Sisaks Zug*, in the *Z.d.d.M.*, T. XI, p. 421.

§ Brugsch, *Geogr. Ins.*, T. II, p. 67.

|| Brugsch, *Geogr. Ins.*, T. II, p. 68.

¶ Joshua xv, 29; xix, 3.

limit of the Jewish territory.\* His opinion was rejected by Blau,† who prefers Etham עיטם, but Rougé‡ pronounces for Azem, which answers in fact more exactly than Azmôn to the hieroglyphic orthography. The identity of the two towns is certain, but the position of Azem is considered undecided by most authors who have studied the Bible. The way in which the text of Sheshonq introduces Azama, a little distance from Migdol-Gad (el-Medjdel), and Iourza (Kh. Ierzah), obliges us to dismiss the hypotheses of those among them who place Azem very far towards the south; that of Wilton, for example, who would recognize only one place in the names Ijîm and Azem in the book of Joshua, and sets it at el-Aujéh, in the territory of the Azâziméh Arabs.

It is probable that this town should be found in the most northern part of Sineon, between Ouady el-Hesî and Ouadi esh-Sheriâh, nearer the former than the latter, since the Egyptian list inserts only five cartouches between Iourza and Azama. The precise place is all the harder to determine, because to meet with a town whose situation shall be free from doubts, we must go down more than forty cartouches lower, to the Canaanite Arad (No. 108). There at least we find ourselves for a moment on solid ground. ערד Arad is the Tell-Arad of our modern maps.§ If we reflect that our list consists of many series of names representing sites little remote from one another, we shall be brought to believe that these forty and odd cartouches, of unsettled value for the moment, should be scattered in more or less regular fashion over the country which extends from the environs of Ierzah to those of Arad. The presence in the preceding sections of many places set pretty close to one another to the north of Ouady el-Hesy leads me to think that the earlier at least of these forty cartouches should be sought either on the very course of this Ouady, or in the mass of hills which border its southern edge. On the other hand the absence of names such as Gêrar and Ber-Sheba seems to prove that the geographical area of the list does not extend very far towards the south. The district to explore is not then so considerable as we should be tempted to believe at first




\* Numbers xxxiv, 4; Joshua xv, 4.

† Blau, *Sisaqs Zug*, in the *Z.d.d.M.*, XV, p. 241.


‡ E. de Rougé, *Mémoire sur l'Origine*, pp. 77, 95.


§ Brugsch, *Geogr. Ins.*, T. II, p. 70.





start; unhappily it is in a part still imperfectly known, in spite of the fine works of the Palestine Exploration Fund, and the maps leave spaces more or less entirely void of names or topographical indications. The identifications there have then a still more uncertain character than they present in the regions already studied.

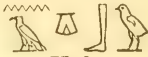
On careful consideration, the forty and odd cartouches are far from representing so many independent localities. Many of them contain, as Brugsch has seen, common names which serve to designate characteristics of the ground. These words, preceded sometimes by a masculine article  *pa*, or feminine  *ta*, form the first part of a name whose second part is inscribed in another cartouche, with or without the insertion of the Egyptian preposition  *n*. The most often employed of these words is, with different spellings,




 (No. 71) *Hougra*,  (No. 77) *Haqra*,

 (No. 94) *Haqri*, once more the same in plural

or dual  (No. 107) *Haqrima*, *Haqraïma*.

Brugsch has seen here the Arabic word  *stone*,\* and I have adopted his interpretation.† It raises an objection of the gravest:  is Arabic, and we are in Hebrew land. The only lawful equivalent of the Egyptian word would be  *hagar*, which is found in some geographical passages of the Talmud, and to which we attribute the sense, *inclosure, wall*, from the root  *cinwit*.‡ We may understand this word of

those great circles of stone, of which many exist still in Arabia Petraea, and of which more than one vestige has been discovered in the parts of the country whither the list of Sheshonq leads us.§ The second word 



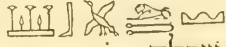

*nagabou* is compared by Brugsch either to  *Negeb*, Hebrew, or to  *nakb*, or 'pass' of the Arabs. The same objection which has been raised against  avails equally against

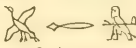
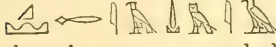




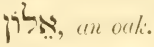

\* Brugsch, *Geschichte Ägyptens*, p. 661-662.

† Maspero, in the *Zeitschrift*, 1880, p. 47.

‡ H. Hildesheimer, *Beiträge zur Geographie Palästinas*, p. 67, *sqq.*

§ See a very clear description in Palmer, *The Desert of the Exodus*, Vol. II, p. 320, *sqq.*

نقب, and it is really נַבֵּב which alone can answer to the Egyptian term.  Δ *amaqou* is עֵמֶק, the valley;  | *ta shodinaou* is an Egyptian term which seems to mean *the canals, the ditches*. As to  *Shabbalout*, where Brugsch has thought to recognize שְׁפֵלָה, *the plain*, which is impossible,  answering to ב, never to פ,—it is שְׁפֵלָה, *a stream of water, a river, a torrent*.\* All these words are in perfect accord with the nature of the country where the localities are placed which they serve to designate: as the names which the lists of Thoutmos III bring to our knowledge, Abilou, Karmona, Ganotou, shew us the fertility of the central territory of Judah,† so do those of the list of Sheshonq bear witness to what was the territory of Simeon.















When we have taken account of the preceding observations, the number of sites becomes perceptibly limited: from No. 65 to No. 110, from thirty to forty-five cartouches are lacking.‡ Here they are in the same order in which Sheshonq has given them to us. (Nos. 65–66)  the Valley of Azama, that is to say probably the valley where lay the town of Azama, and the town itself: site unknown. (No. 67)  *Anara* or *Anala*, not *Anali* or *Anari*, for the fashion in which  is cut makes me believe that the sculptor, not having kept room enough for the  final, was content to insert  before . This name is one of those, very rare they are, of which I do not find the Semitic equivalent. Perhaps it contains a mistake, and we should read *Alouna*, which will give us a regular form , *an oak*. Anyhow the site remains unknown. (Nos. 68–69) 

\* Maspero, in the *Zeitschrift*, 1880, p. 47.

† See some observations on this subject in the *Transactions of the Victoria Institute*, Vol. XXIII, p. 67 and 74.

‡ Henceforth I shall rarely cite Blau, since he takes each cartouche for a complete name, the perpetual repetition of words of which I speak has led him into almost as many errors as there are cartouches remaining to the end of our list.

 *Pa hagra-fit-iaousha.*

The name presents a rather curious peculiarity.  is here for  by substitution of , *v. f.* for  **ב**: we shall see further on that the **ב** is transcribed  *oua* in several words. These variants are not due to the caprice of the Egyptian scribe. I think they reveal to us a new fact in phonetics. It should seem, according to them, that in the Hebrew of the race of Simeon the **ב** was weakened to *v, f*, at least in certain cases. *Fit-iaousha* is then the transcription of a **בֵּית־יוֹשָׁה** *Bêth-Ioshah*, of which the former element is slightly disfigured by a pronunciation *Vit, F'it*, for *Bit*. The term joined with **בֵּית־** is not, as I have believed, **אֵשׁ** *fire*: The division   *Fiti*,  *aousha*,\* which supposes this identification, is not admissible in our list, where  is always written without  final. I divide   *Fit*  *Iousha*, and take *Iaousha* for the transcription of **יֹשָׁה** *Ioshah*. This name designates, in the first book of Chronicles† one of the Simeonite chiefs who, in the time of Hezekiah, emigrated to the East from the town of Gedor in search of pasturage for his herds. Whatever may be the value of the information furnished by the Chronicles, it shows us at least that the name *Iaousha* was used in the tribe of Simeon, and confirms the legitimacy of the transcription **בֵּית־יוֹשָׁה** *Bêth-Ioshah* which I have adopted for *Fit-Iaousha*. To recover in the present nomenclature an equivalent of this locality, we must not forget that the dialectic phenomenon caught by the ear of the men of Sheshonq's time, must have perpetuated itself among all the populations who have succeeded in these parts. A name once adopted in one pronunciation passes with this pronunciation to different generations who have used it daily. The Arabic equivalent of **בֵּית־** initial ought then to be something analogous to the Egyptian  *vit, fit*, either **فَيْت** or **فِت**. I find, in fact, a little to the south of the Oued esh-Sheriah, a *oued* and a ruin which

\* Maspero, in the *Zeitschrift*, 1880, p. 47.

† 1 Chronicles iv, 35.

Guérin calls *وادي فتيس*, Oued Ftis and *خربة وادي التتيس* Kharbét Oued el-Ftis,\* and which the English map spells Kharbét Futéis. *فتيس* contains all the elements of *فت*: *فت* is here the counterpart of *بيت* = *بيت*, and *يس* answers to *יזשה* by that substitution of *ס* for *ש* which is so frequent in the transition of Hebrew names to Arabic. The identification of Fit-laousha with Kharbét Foutéis, Fatéis or Ftís confirms the opinion which I have above expressed on the subject of Azamah, and encourages me more and more to seek this town between Ouady el-Hesy and Oued esh-Sheriah, rather near a line drawn from Kharbét Foutéis to el-Medjdel, or to Kharbét Ierzah.

It is equally in the neighbourhood of Kharbét-Foutéis that it would be worth while to seek the towns which follow, if the maps were not so desperately poor. *Arou-haloul*, *Alou-haloul*, is one of those names in which Rougé would recognize the Arabic article *ال*; † I have already said above ‡ how difficult it is to allow in our list the presence of Arabic forms, and I will not repeat it. I think that we have here one of those names *אל-הלל* *El-halal*, in which are present both the word *אל* (*God*), and the root *הלל* *lavit*, *splenduit*. The site is unknown to me. (Nos. 71-72) *Pa hougra abilama* contains a plural *אבילים* from *אביל* *meadow, field*: the whole is translated *the enclosure of meadows*, and designates an unknown site. *Shabbalout ni Gabri* (Nos. 73-74) literally signifies *the river of the Hero*, for *Gabri* is the exact transcription of *גברי* *man, soldier*. Blau has identified the second cartouche with Betogabris, which is now Beit-Djibrin, § and I adhered to his opinion, || but

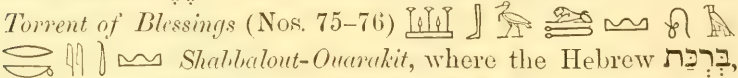

\* Guérin, *Judée*, T. II, p. 287.

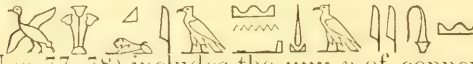


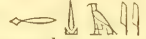
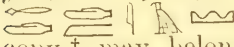
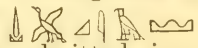

† E. de Rougé, *Mémoire sur l'Origine*, p. 90.

‡ See above, p. 18-19.

§ Blau, *Sisaks Zug*, in the *Z.d.d.M.*, T. XV, p. 23. He read the name *Ngbarij*, "wobei das *x*, entweder bloss lautlicher Vorschlag ist, oder graphisch richtiger am Ende der Cartouche zu stellen sein wird."

|| Maspero, in the *Zeitschrift*, 1880, p. 47.

Beit-Djibrin is too distant from the country where the vicinity of Kharbét Foutéis obliges us to remain, for allowable persistence in this opinion. I thought for a moment of Azion-Gaber, עֲצִיּוֹן גַּבֵּר, but this town is too far toward the south. I will rather seek our village of Gabri and its stream in the vicinity of el-Gabra الجبري, a little to the east of Oumner-Roumanin. El-Djabri is in fact the Arabic equivalent for the Hebrew גַּבֵּר. *The torrent of the Hero* is followed by the *Torrent of Blessings* (Nos. 75-76)  *Shabbalout-Ouarakit*, where the Hebrew בְּרִכַּת, בְּרָכָה, plural בְּרָכוֹת, has its ב transcribed  *Oua*, according to the dialectic pronunciation which I have indicated above.\* This locality is certainly different from the Valley of Blessing עֵמֶק בְּרָכָה of Chronicles,† but the site is uncertain.

 *Pa haqra n-Azai* (Nos. 77-78) includes the  *n* of connexion which we have already had in *Shabbalout ni-gabri*; the determinative of the flame  belongs to the sense of the Egyptian and not to that of the Semitic word:  *azai* belongs either to עֵז *goat*, or to עֵז, עִז, *strength, power, fortitude*. Site unknown.  (No. 79) *Adidima*, according to Brugsch's copy,‡ may belong to the root עָדָה *induit ornamentum, ornavit se*: this will then be a form analogous to that of עֲדִיתַיִם *Adithaim* (double adornment) which designates a town of Judah. I am not aware of the site.  *Zapaga* (No. 80) is the transcription which is admitted since Brugsch proposed it, although indeed it shows no connexion with any root known in Hebrew. Perhaps it may be permitted to divide the word in two, the former part will be a derivative from the root צָפָה *speculatus est, prospectavit*, and the latter גֵּיאַ *valley*, but all this is too uncertain to dwell upon. The two names that follow are helplessly mutilated, but No. 83  *Ganat* gives us a well-known term גַּנֵּד *garden*; it is perhaps the Kharbét Jenneta خربة جنتا, which is situated at some distance to the south-west of

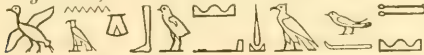
\* See above, p. 20.


† 2 Chron. xx, 26, *cf.* in Josephus, *Arch.*, ix, 1, 3.

‡ Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 79.




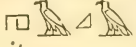
el-Daouaiméh. In this case the localities intermediate between Shoubbalout ni-Gabri and Ganat should be sought, partly in the valley of Oued en-Nâs, partly in that of Oued el-Djezair, from south to north.

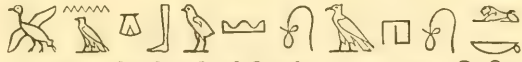


 (Nos. 84-85) *pa nagabou âzahout* furnishes us a new example of a name which the scribe has disfigured by giving it an orthography that resembles an Egyptian word: he has decomposed *Azahout* into two terms, of which the former became for him the verb

 *to spoil, to rob.* As there is no such root as *עזח* or *עצה*, I am tempted to see here one more compound whose first member will be *עז, עז* *strength, fortitude.*


The names which follow it no more lend themselves to a reasonable identification than this, and it will suffice for me to enumerate them.

(Nos. 86-88)  *ta shodinaou pa hagali Shanaïa*, which ought to be translated, *the canals and the enclosure of Shanaïa*: *Shanaïa* is probably a derivative of the root *שן, שפן* *quievit.*


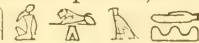


(No. 89)  *Haqa* belongs to *הקה* *murmuravit, susurravit.*


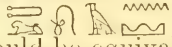
(Nos. 90-91)  *Pa nagabou ouaha-ouarouk*, the land of Ouahaouarouk. *Ouarouk* is here a dialectic form of *Barouk*, as we have seen above: the  preceding may be the article *ה*, but I do not see to what  *oua* can answer.






(Nos. 92-93)  the land of Ashakati. *Ashakati*, with the determinatives , is a derivative of *שקה, שחקה, שיקה*, *a pit.*

(Nos. 94-95)  The enclosure of Haninia, is certainly not the Beît-Hanîna of the neighbourhood of Jerusalem; \* it is a locality of Simeon or of the southern districts of Judah. *Haninia* is derived from a form of *הנן* and appears to answer to *הנניה*. The name

\* Brugsch, *Geogr. Ins.*, T. II, p. 69.

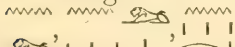
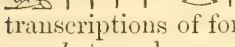






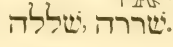



recurs at No. 99  Hananii: there were then, not far from one another, an enclosure (*Hagari*) of Haninia and a town of the same name, as we have further on the enclosures of Arad and the town of Arad (Nos. 108-111). Between these two places, or beside one of them, we meet with (No. 96)  *Alaqal, Alouqad*, and (No. 97)  *Adimain*. The former name includes perhaps  $\text{גַּד, גִּרְד}$ , *incidit, irrupit*:  $\text{גַּד-גִּרְד}$ ; the latter belongs to  $\text{גַּד-גִּרְד}$ , *red*. All these places are rather near to Doura, for No. 100 is  Adora the Great, which is identical with Adoraïm and with Dourah.\*

(Nos. 101-102)  the enclosure of Toul-Zan; Brugsch reads  *Toul-Ouan*, which, with the  $\text{ou}$  for  $\text{b}$ , would be equivalent to Toul-Ban, Ban being perhaps  $\text{בַּן}$  *son*.† The doubtful sign has now entirely disappeared, as I have had occasion to ascertain at Karnak: it is not then any longer possible to know what was the true reading.

(No. 103)  *Haideba* comes from the root  $\text{הַיַּב}$ , *cecidit lignum*, which under the form  $\text{הַיַּב}$  means *cecidit, excidit lapides*: the Egyptian spelling supposes a variant  $\text{הַיַּב}$  which the Hebrew has not preserved.  $\text{הַיַּב}$  *Haideba* would signify a place where they hew stone, a quarry; as the word recurs below (No. 105), we ought perhaps to recognize in it a common name as  *naqabou*,  *Hagara*, which we should join to the cartouche following.  (Nos. 103-104) *Haideba Sharounram* and  *Haideba Diouati* will then be the quarries of Sharounram and

\* Maspero, *Sur les Noms de la Liste de Thoutmos III qu'on peut rapporter à la Judée*, *Trans. of Victoria Institute*, Vol. XXII, p. 63-65.


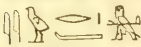

† Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 102.


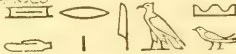




of Diouati. The former name Sharoumram seems at first to be foreign to the Semitic languages; but the combination   *nr* in Egyptian has not, at least in the transcriptions of foreign names, any other value than that of *r* or *l* strongly pronounced. *Sharoumram* is then a derivative either of the root  *traxit, extraxit, spoliavit*, or of the root  *torsit, firmus, durus fuit, oppressit*. The termination  may answer to ים of the plural; but this inflexion is almost always written in our list with a final vowel  *má*, and without י. I think rather that there is here a mistake of the cutter, and that we should read  instead of : we should then have to do with a name .  *Diouati*, derived from the root  *amavit*, whence the name of king David, or from the root  *lanquit, ægrotus fuit*. Neither of these localities has left any still recognizable traces.


Some may perhaps be astonished to see me indicate so minutely the Hebrew roots to which this and that name of our list seems to me to answer. This is not affectation of philological research: it is, I believe, an indispensable precaution in the hazardous sort of study to which I have been bound to devote myself. One is only too prone to suppose an error of the copyist, a mistake of the scribe who has compiled the list, and to invert the order of the letters to obtain a comparison with an ancient or modern name already known. In shewing that the Egyptian letters transcribed in Hebrew letters yield regular or possible Hebrew forms, I avoid for myself, and perhaps for scholars who will treat this subject after me, the temptation to attribute to an error of the scribe the presence of so many unknown names, and the fault of modifying these names by inversion or by substitution of one articulation for another. If our transcriptions in Hebrew letters give us regular words, it is because the Egyptian scribes reproduced as exactly as their alphabet allowed them the sounds that they heard in Judæa: we have not then the right to make any change in their transliteration.

Nos. 108–110 furnish us with the first absolutely certain identification which we have in this part of the list:

   *Hagaraim Arada*, the

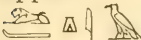

two inclosures of Arad, and  *Rabbit Aradai*, רַבֵּת עֶרֶד. The two inclosures of Arad were in the vicinity of the town of Arad, and perhaps a more complete exploration of the country will help us in our time to discover the traces. I scarcely need recall\* that Rabbat Arad is עֶרֶד of the Bible, now Tell Arad, تل عراد. (No. 112)  *Iaourahma*, appears to me to be identical with יֶרַחְמֵאֵל Ierahméel, of the tribe of Judah, the *Négeb* of which was pillaged by David. אֵל is wanting at the end of the Egyptian name, but it is also wanting at the end of the Arabic name Ouady Rahaiméh, واد رحيمه, which has succeeded to the Hebrew name: the omission of the divine name, which has permitted the Arabs to transcribe as they have done, is doubtless a primitive occurrence, since we find יֶרַחְמֵאֵל *Iaourahma*. No. 111  *Nebatout*, placed between Rabbat-Arad and *Iaourahma*, should be sought between Tell Arad and Oued-Rahaiméh, but none of the names known hitherto enough resemble it to give room for identification. *Nabatout* appears to be a plural נְבָטוֹת of a feminine word נְבָטָה, which would be derived from the root נָבַט *conspexit, vidit*.

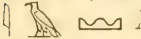
Three cartouches have entirely disappeared after *Nebatout*. No. 116  *Ari... m...*, is too mutilated to yield anything. No. 117  *Adora-shirau*, *Adora* the little, by antithesis to the *Adora* the Great of No. 100, brings us back to the north of Ierahméel and Arad, probably in the part of the mountain land of Judah which stretches south of Hebron, but I cannot pretend exactly to determine the site. No. 118, read by Brugsch  *Pabia*,† but whose first sign is left by Champollion undecided, has appeared to me to be rather  *Zabia*: nothing is in fact more easy than to confuse a damaged  with a .



\* Brugsch, who was the first to recognize Arad, has separated from it the cartouche  which he reads *Lebat*, and identifies with לְבָאוֹת בֵּית לְבָאוֹת *Bêt-Lebaoth* of Simeon (Brugsch, *Geogr. Ins.*, T. II, p. 69-70).



† Brugsch, *Geogr. Ins.*, T. II, pl. xxiv, No. 118.



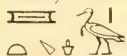
This reading is so much the more likely that there is no root פב, but many roots such as צבא, *prodiit, processit miles in bellum*, whence צבא *army*, צבה *prodiit stella*, &c.: צביה would be the exact transcription of צביה *a doe*, a name given to women, צביה, צביה, and which may be here applied to a locality. No. 119 is read by Champollion


 *Rahouga, Lahouga*, by Brugsch 

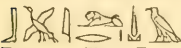


 *Mahouga*. As the roots רחק *longe discessit*, לחך *linxit*, פחך *percussit*, are there to support whichever reading we may prefer, only the study of the wall will permit a decision between Champollion and Brugsch. It is now so damaged that I am not able to recognize anything in it; perhaps someone else may be more happy. No. 120 is evidently

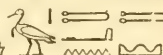
 *Ouariouk* with  *oua* for ב, as above, either a new name derived from the root ביר, perhaps the Kharbét Barouk, خربة باروك, of Guérin. (No. 121)

 *Fir-timda*, includes, as  *Fir*,\*

above, a dialectic form *Fir* for באר, ביר *the well*: the name will be באר דמעה *Ber-Dimeh*, the well of the tear,  having often in Egyptian the value of ד vocalized with *i*, and  equivalent to מע. Names beginning with *Bir* are frequent at present in the desert region which extends between Hebron and the Dead Sea, but none of them recalls our Ber-Diméah. (No. 122)  *Abilon* is an unknown

 *Abil*, situated in this region of the desert of Judah. (No. 123)

 *Biar-Rouza, Biar-Louza*, is באר לוז *Ber-Louz, Ber-Louzah*, the well of the almond tree, with the ordinary orthography in B  and no more in V, F .


This *well of the almond tree* has nothing in common with the two places of the Bible called Louz, one of which was the Bethel of Benjamin, and the other belonged to the Hittites of northern Palestine. The following name is 



*Bit-Anati*, which I will identify, as Brugsch does,† with the


\* Guérin, *Judée*, T. III, p. 164.


† Brugsch, *Geogr. Ins.*, T. II, p. 70.


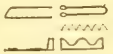
בֵּית־עֲנוֹת Bethanoth of Judah, now Bêt Anoun, خربة بيت عنون. Ber-Louzah ought to be found some way S.E. of this site, on the slope of the mountains which descends to the Dead Sea.

Bit-Anat is the last name of the list which we can set on the map with certainty. Besides, no more exists than two cartouches intact, and the remains, more or less legible, of half a dozen cartouches. (No. 125)  *Shalhatou*, belongs

to the root שָׁלַח, *misit*, whence comes the name שָׁלַח, שְׁלַח of the pool of Siloéh. The group  which ends the word is the plural *toou* of the word  *to*, land, and could scarcely be employed here, in the transcription of a foreign word,

except to render the syllable *tu*, *toou*: as  *Shalaha* furnishes us already with the three letters of the

root שָׁלַח, the final *tu* is a grammatical inflexion, and the equivalent of  $\text{TI} \equiv \text{T}$ , which serves to write the termination of the feminine plural. שְׁלַחֹת, *sprouts*, answers exactly to *Shalahatou*. The book of Joshua makes known to us a town of analogous name, שְׁלַחִים (*armed men*), situated in the southern part of Judah.\* The modern site of *Shalahatou* is not known to me. (No. 126) 

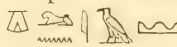
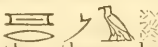
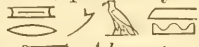

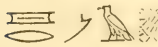


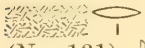
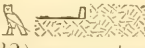

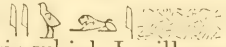

*Alomâten* or *Armâten* is a name composed of  *אל* and , which may answer to מִדִּין *Middin*, or to any

other form of the root מִדִּין *reperere, moderari*; I have cited מִדִּין in preference, because it is the name of a town of the tribe of Judah.† As Middin was in the vicinity of the Dead Sea, that is to say in the region where we know that the last cartouches of our list are situated, we may ask ourselves whether it is not identical with our Alamaten; Middin would then be an abbreviation of a more complete form el-Middin. We do not know from any other source the situation of Middin: Sauley‡ alone fixes it at قصر مردة Qasr Mirdéh, the

\* Joshua xv, 32.

† Joshua xv, 61.

‡ F. de Sauley, *Dictionnaire topographique de la Terre-Sainte*, p. 223 s.v. *Meddin*.

حربة مرد Kharbét Mird of the English map, Mird مرد of Robinson,\* which will sufficiently agree with the place which Alamâten occupies in our list. (No. 127)  Galouna, Garouna, is certainly not, as Brugsch thinks,† the גולן Golan of Manasseh, which is too remote from the country to which the list obliges us to keep. It seems to me that we have here an exact transcription of the word גרנה, גרן, threshing-floor: I do not find on the map any place named جرن Djarn or جرين Djarin, which would be the Arabic form of the ancient name.  (No. 123) Alama . . . , Arama . . . , including the three letters of a root אלם, cannot be completed except by a suffix, probably that of the plural, either the masculine plural  Alama, or the feminine plural  Alamat. It is impossible for me to say which we should prefer here, for אלה a sheaf, a bundle of corn, which is the prototype of  Alama, has both plurals אלהים and אלהות. The following name, which Brugsch gives thus  . . . lahat or . . . rahat (No. 129), appears to have been , either ארחות, plural of ארה, way, path: I do not know where this locality was situated. Nos.  . . . la (No. 130)  Ma . . . (No. 131),  El . . . (No. 132) are not susceptible of any interpretation. The last  (No. 133) Iaoura . . . affords an hypothesis which I will content myself with indicating summarily, after Blau. If we complete it  Iaourishalama, we should have here ירושלם Jerusalem, which with good right we should be astonished to miss among the towns taken by Sheshonq.

Such is the result of this study. The gaps with which the list is riddled do not permit me to affirm with certainty

\* Robinson, *Biblical Researches*, Vol. II, p. 270.

† Brugsch, *Geogr. Inschriften*, T. II, p. 70.

that the important towns of Judah or of Philistia whose names have not been mentioned, Ascalon, Ashdod, Gath, Gezer, Hebron, Jericho, &c., did not figure there in fact. The lists of Thoutmos III, which are intact, present so many omissions of this kind, that I should be quite ready to believe, for my part, that they were lacking really in that of Sheshonq, and that the cartouches now destroyed may not originally have contained, for the most part, any names but those of insignificant localities as obscure as those whose memorial has been preserved to us. Sheshonq had a definite surface of wall to cover, and wanted for that purpose a determinate number of names. The despatches of his army and the reports of prisoners or of allies furnished him the number of which he had need. We will say that the scribes had less the intention to enumerate the principal towns of their new conquest than to mark its outline: the places which they chose form round Jerusalem and the block of the country of Judah a sort of circle which seems to follow pretty exactly the frontier of the kingdom. Many of them are quite unknown, others are only identified under all reserve with some Hebrew or Arabic names; the smaller number are placed on the map in an indubitable manner. My work is here but provisional; I hope to resume it hereafter, or, if I am unable, others will take it up and carry it further.

The CHAIRMAN (Professor E. HULL, LL.D., F.R.S., F.G.S.).—Our thanks are due in the first place to M. Maspero, the author of this paper, which is one, as you can judge for yourselves, of considerable difficulty and research; in the next place to Mr. Pinches, who has read it so ably; and, I have also to ask you to return your thanks to the member who has been kind enough to translate it from the original French, viz., the Rev. H. G. Tomkins, of whose translation of M. Maspero's former papers on the names on the *List of Thothmes III* (see vols. xx and xxii) the author said, "il etait aussi fidèle et aussi élégante que possible." (Applause.) I will now ask that the communications received in regard to this paper may be read.



The following communication was then read:—

*Notes on Professor Maspero's Paper on the List of Shishak.*

From Major C. R. CONDER, R.E., D.C.L., LL.D., &c.:—

Professor Maspero's valuable paper throws light on a list which was previously very obscure. I began to study the list of Shishak's conquest of Palestine in 1879, and I thought the whole, as published that year in Brugsch's *History of Egypt*, very difficult, so that I only ventured to publish a few identifications, some of which did not agree with his. In two cases M. Maspero supports my view. He began to study the question next year as appears from his paper. His amended copies of some of the names explain many difficulties. Generally speaking it seems clear that the list begins with the country between Gaza and Megiddo, and goes south, along the Philistine plains and low hills to the east. It then enumerates places in the Beersheba deserts, and it returns north by the Hebron hills, perhaps to Jerusalem.

One or two general remarks may be of use, before considering the towns in detail, when I think I may be able to reinforce M. Maspero's general view, by some new proposed identifications which he does not notice. Though we are in a Hebrew country it does not follow that the names of the towns are strictly speaking Hebrew. The Canaanites, as shown by the Tell Amarna letters, spoke, from 1500 B.C. downwards, an Aramaic dialect. The old town nomenclature was unchanged in most cases by the Hebrews, and in the present list, as in that of Thothmes III, there are indications that the Egyptian scribes followed the Aramaic rather than the Hebrew forms of the words. This is specially marked in the terminations in *u* which was the nominative in Canaanite, as in Assyrian, but not in Hebrew.

The order is no doubt roughly consecutive, and M. Maspero has very properly rejected names which have been suggested in distant regions, for others which are near each other; but it is not always very certain what the order is in detail; and an identification may be missed by not looking widely enough on the map. In 1880 the Survey Memoirs were not published, and M. Maspero has not referred to them. This has led him into one or two minor errors; and I am sure he would not have brought the charge which he makes against my Survey, if he had personally visited the region, or had read the memoirs. On page 110 he says, "the

maps leave spaces more or less entirely void of names, or topographical indications"; but the district to which he refers is an open pastoral desert, with a few arable tracts, in which there are neither ruins nor springs, and in which—as in the desert of Judah and in Moab—names are few, and no traces of ancient settled population occur. This region was quite as carefully surveyed as others, and cannot be said to be "imperfectly known." Want of acquaintance with the country has led M. Maspero to make a statement which I am obliged to notice, because he has unintentionally brought a charge against the Surveyors, which I am certain he would not have made if he had read the account of the country in the Survey Memoirs; and which if unnoticed might mislead others. The hills, valleys, and natural features in this district are given with the same amount of detail as in the thickly populated parts of my Survey.

To proceed to the details of the list, which (as concerns Palestine) begins with No. 11 Gaza and No. 12 Megiddo, the towns are as follows:—

- No. 13. *Rabbati*, Rabbith, follows Brugsch. The site I have placed at the modern *Raba*.
- No. 14. *Taanaku*, Taanach, follows Brugsch. The termination in *u* here denotes the Canaanite form.
- No. 15. *Shavnama*, Shunem (Brugsch) is now *Sâlem*.
- No. 16. *Bit Shanla*. It seems to me that Shiloh is too far away, and Brugsch's Beth Shean more probable.
- No. 17. *Ruhaiba*, *Tel Rehab*. M. Maspero adopts the identification which I proposed in 1879 with the Roob of the Onomasticon, and discards Brugsch's suggestion of Rehob, which is far away to the north.
- No. 18. *Hapurama*, Haphraim, as Brugsch proposed in 1879, I believe to be the modern *El Farrîyeh* west of the plain of Esdraelon.
- No. 19. *Adulmim* could certainly, as M. Maspero says, not be Adullam. Perhaps it might be Idalah of Zebulon, the later Hirii (Talmud of Jerusalem, *Megillah* 1) which appears to be the modern *Huvarah*.
- No. 21. *Shawadi* might be *Suveidiyeh*, as M. Maspero proposes, since the ruin seems to be ancient, but the Arabic *s* does not usually represent the Aramaic or Hebrew *sh*. I am inclined to think the real site was Shadid (Sarid in the A. V.) which is the modern *Tell Shadûd*.

- No. 22. It seems to me doubtful if Shishak went over Jordan, and as no other towns are noticed beyond Jordan, I think we should not place *Mahanema* at Mahanaim—which I have identified north-east of es Salt, but more probably in the *Mukhnah* plain, near Shechem, which would agree with M. Maspero's view that No. 20 is Shechem itself—only No. 20 is unfortunately erased.
- No. 23. *Kebeana* is no doubt Gibeon as Brugsch proposed in 1879.
- No. 24. *Bit Huarun*. No doubt Beth Horon (Brugsch).
- No. 25. *Kadutim*, according to M. Maspero, is *Kademoth*, according to Brugsch. I am inclined to think that *Katanneh* is the modern site, being near the places with which this name occurs (قطنة). The Egyptian lettering does not forbid such a proposal.
- No. 26. *Aiaulun*. Ajalon, according to Brugsch, is mentioned also in the Tell Amarna texts.
- No. 27. *Makidan*. Makkedah. M. Maspero confirms the suggestion which I published in 1879, as against Brugsch's suggestion of Megiddo. The site is noticed in the Tell Amarna tablets, with topographical details which fully confirm Sir C. Warren's proposed identification with *El Mughâr*, "the caves."
- No. 28. *Adiru* or *Adilu* may, I think, perhaps be (Ataroth) Adar, which I discovered at the modern *Ed Darich* close to Beth Horon.
- No. 29. *Yudah maluk* seems to me correctly fixed by M. Maspero. When in 1879 I proposed the site of Jehud (*El Yehûdiyeh*) I was not aware that Dr. Brugsch held such a view, as he gives no identification in his list in 1879. This opinion seems to me much more probable than the old suggestion "King of Judah," which is contrary to Semitic syntax. The name bears the sign for "country," not for "person."
- No. 31. *Haianim* (or *Haanma* according to Brugsch). I think the ruin *Hannânah* is too insignificant to be the site of an ancient town—a few traces of ruins only remain with fig gardens—and that *Beit 'Anân* is more probable. The interchange of *Ain* and *Cheth*, which is not uncommon in the modern peasant dialect of Palestine, seems clearly to have been also an Amorite peculiarity, according to the Tell Amarna tablets, and was also a Samaritan vulgarism.

- No. 32. *Aluna* might perhaps be softened into *Elon* in Hebrew. The site I think is the modern *Beit Ello*, which comes in the proper position.
- No. 33. *Bilumam* or, according to Brugsch, *Bilema*, I should propose to place at *B'alín* west of *Tell es Sâfi*.
- No. 34. *Zaidi Putir*: perhaps the first word is the Aramaic *Sadeh*, "mountain." The names of the ancient sites often remain at springs, and I would suggest *'Ain Fatir*, a spring in the hills east of the last. The little village of *Sidún* is marked on my survey.
- No. 36. *Bit 'Alemat*. The site at Alemeth seems to me rather far east for the places which precede and follow, and, though the question may be one of opinion, I think *Beit 'Alam* fits better for locality.
- No. 37. *Kegali* might in this case be the ancient Keilah mentioned on the Tell Amarna tablets, as well as in the Bible: now *Kilah*. The interchange of *g* and *ain* seems, from the tablets, to have occurred in Amorite speech, which confirms M. Maspero's view as to the word, though not as to the site.
- No. 38. *Shocoh* as proposed by Brugsch in 1879 fits with the preceding.
- No. 39. *Bit Tupu*. There appears to have been a town called Tabu not far from Hebron, noticed in the Tell Amarna tablets, and to be placed I think at the ruin *Taiyibeh*, north-west of Hebron. This would fit for No. 39. The places which, like Deir Dubban (more correctly *Deir edh Dhibbân*) are called after a *Deir*, or "monastery," generally took the name I think in Christian ages. The caves at this site have Cufic inscriptions, and like those at Beit Jibrîn, seem to have been excavated in the middle ages. The word *Dhibbân* could hardly represent *Tupu*, since the *Dh* is the proper equivalent of the Hebrew *Zain*.
- No. 40. *Abirau* might I think be the ruin *El Bireh* further west, which I believe to be No. 99 of the list of Thothmes III.
- No. 53. *Nupilu*. If this come from the root *Nup*, "to be high," I think it must be the present *Nuba*, rather further east than the preceding sites, which I identify with the second Nob of Nehemiah (vii. 33).
- No. 54. *Dushati* I should be inclined to place at the ruin of *Tauwûs* south of *Nûba*.

No. 55. *Pauru Kitut*. It is to be noted that the word *Pauru* for a "chief," is used in the Tell Amarna tablets, and Gath is fixed by their statements at *Tell es Sâfi*, and called *Giti*. The Gittites are mentioned in these tablets; and I would therefore advocate the view which M. Maspero mentions, and render this name "chief of the Gittite regions." The plural in *utu* for masculine and for feminine occurs in Assyrian and in Amorite, recalling the curious Hebrew form *Aboth*, "fathers."

No. 56. *Adima* (or *Adoma*, Brugsch) I think must be *Ed Duweimeh*, a village south of the preceding.

No. 58. *Magdilu* is no doubt rightly placed by M. Maspero, and the same as No. 71 of the list of Thothmes III.

No. 59. *Iarza* is also no doubt right. The name of the ruin near *Mejdel* was collected by my scribe as *Erzeh* not *Yerzeh*.

The latter part of the list, with its cartouches defining districts followed by names of places in each, is much elucidated by M. Maspero's new work. I think however a few sites may be added to those which he proposes.

Nos. 65-66. "Azmon in the Valley" carries us south of Beersheba.

No. 67. *Anari* might perhaps be corrupted into 'Omri, the name of a ruin north of Beersheba, which would fit with the next.

No. 68. *Pi Hakarau Pitiausha*. I believe M. Maspero fixes an important point in suggesting *Futeis*, which is a large ruin. I have proposed to identify it with the Pitazza of the Tell Amarna tablets. I would suggest that *Hakarau* may be for 'Akarau (the 'Ain and *Cheth* being undistinguished), and that it means the "barren" or "unproductive" district. All the places so defined lie in the deserts near Beersheba.

No. 70. Brugsch suggests Aroer, which seems not impossible. It is not the maps that are "poor" in this district, but the country which is desert, with very few habitable sites.

Nos. 71-72. *Pi Hakarau Abilama* (or *Abiroma* according to Brugsch), another desert site. I cannot understand how *ma* can be regarded as a plural in any Semitic language.

Nos. 73-74. *Shabbalut ni Gabri*. I think M. Maspero identifies this in a most probable manner at *el Jâbri*. The site is now a ruin with caves, for there is not a single inhabited village in this region. The Amorite plural, like the Assyrian, end

- in *z*, even when not in the construct case. The word would therefore seem to mean "stream of giants." North of Beersheba.
- Nos. 75-76. If we are to read *Shabbalut Barakit* "stream of the tank," the reference might be to the stream at Beersheba, close to the last.
- Nos. 77-78. *Pi Hakarau-n-'Azai* should be sought further south.
- No. 79. *Adidima* I should suppose to be *Adadah*, which preserves its name south-east of Beersheba. The *ma* seems to be a definite pronoun added in this and other cases, as in Assyrian and Amorite.
- No. 80. *Zapaka* suggests the word *Tubk*, common in the Syrian dialect for a "plateau."
- No. 83. *Ganat* is no doubt *Jennata* as proposed by M. Maspero. I think it is No. 70 of the list of Thothmes III.
- Nos. 84-85. *Pi Nagabu Azamut* (according to Brugsch), if correct might be connected with *Azmon*, which was in the Negeb or "dry" land.
- Nos. 86-87-88. "The canals and deserts of *Shunaia*" suggest a possible change of *n* for the Hebrew *m*, and in such case with *Shema*, or not impossibly the important ruin *Samah*, north-east of Beersheba.
- No. 89 is perhaps beyond the Survey limits on the south.
- Nos. 90-91. *Ouaruk* if to be read *Bârûk* is I think the ruin so called south-east of Hebron, which, as collected for me in 1875, is spelt with the *Koph* (باروق). It is within the Negeb, which included the desert hills in this part.
- Nos. 92-93. *Ashakati* is *Ashahathat* according to Brugsch. If this be, as M. Maspero holds, from a root *Shukhah*, it seems probable that the village *Esh Shiûkh*, north-east of Hebron, is the site, which would fit with the preceding.
- Nos. 94-95. *Pi Hakarau Hanina*. I think that the ruin of *Ghanaim*, close to Bârûk, is worth consideration. The two gutturals are sometimes interchanged.
- Nos. 96-97. *Pi Hakarau Alagal* or *Arukad*. There is a ruin *Rukah* close to the preceding (رقة) which has the required *koph*.
- No. 98. *Adamaim* (or *Adomam*, Brugsch) might be the large ruin *Domeh*, some miles further west.
- No. 99. *Hanini*, perhaps the Biblical *Anim*, now *Ghuwein*, south-east of the preceding.

- No. 100. *Adorau* would come naturally at *Dûra*, the Bible Adoraim, where M. Maspero places it.
- Nos. 101-102. *Pi Hakar Tulban* would probably be the ruin of *Dilbeh*, near the next.
- Nos. 103-104. *Haidobaa Sharun ram*. The important ruin of *Hadab* ("the hump") would fit well; it is close to *Dura*, about two miles to the south. The term *Sharun Ram* seems to mean "the high plateau," which fits with the position of the site.
- Nos. 105-106. *Haidoba Diuati*, perhaps is connected with the name of *Yuttah*, an important village to the east (Bible *Juttah*).
- Nos. 107-108. *Hakarima 'Arada*, "the desert of 'Arad." <sup>z</sup>
- Nos. 109-110. *Rabbit 'Aradai*, "the capital of 'Arad," as given by M. Maspero, carries us further south, into the Beersheba desert.
- No. 111. *Nebatut* might be *Inbeh*, north of Arad.
- No. 112. *Iaurahma* or *Ibrahma*. Whether or no Jerahmeel be *Wâdy Raheimah*—which may be only named from the *Rahâmeh* Arabs—I am inclined to think that the list goes back, at this point, to the region north of *Dûra*, and would suggest the ruin of *Baârneh* as a corruption of this name; in this case the defaced portion may refer to a suburb of *Dûra*; as M. Maspero proposes for No. 117, "little Adora."
- No. 118. *Zabia* is perhaps the important village of *Sâfa*, north of Hebron (صافا).
- No. 120. If this be restored *Baruk* I think it must be the Berechah of the Bible, now *Breikût*, which is east of the last, written with *cap*.
- No. 121. *Fretima*. There is an important spring called *'Ain Fâris*, north-west of the last. The *t* is sometimes softened into *s* by the Palestine peasants.
- No. 122. *Abil*. Perhaps *Habeileh*—the name of a ruin near the last—is a corruption of this name.
- No. 123. *Bar Loza*. There is a valley called *Lôzeh* in this vicinity further east, which would support M. Maspero's views as to this word.
- No. 124. *Bit Anati* (Brugsch) is now *Beit 'Ainûn* rather further south than the preceding.
- No. 125. *Sharhatau*, perhaps *Siâir*, near the last, or *Beit Shair*,

further north. The inversion of the guttural does occasionally occur.

- No. 126. *Armaten*, "the two Armahs." There are two ruins called *Er Râmeh* west of Beth Anoth, which would agree well.
- No. 127. *Galuna* or *Galena* (Brugsch) might be the ruin *Jâla* further north than any of the preceding.
- No. 128. *Aroma* or *Alama* perhaps 'Alîn, north-west of the preceding.

These suggestions would lead us naturally towards Jerusalem, which M. Maspero considers to have been last on the list, the four defaced names being in the vicinity of Bethlehem.

No. 129. . . . *lhath* might be restored *Malhah*.

No. 130. . . . *raa* perhaps Ephrath (Bethlehem).

No. 131. *Ma* . . . Perhaps Maarath now *Beit Ummâr*.

No. 132. *Ari* . . . Perhaps Kirjath Arim now 'Erma.

No. 133. *Iura* . . . Jerusalem according to M. Maspero.

I think the learned Author is to be congratulated on having made this valuable list far more intelligible than it was, and in having set aside several misleading proposals. I would venture to add that he would find more names on the one-inch Survey than on the smaller map published by the Palestine Exploration Fund. As regards the distribution of names, many sheets of the Ordnance Survey, in the Highlands of Scotland, contain fewer than are shown on my Survey in parts of Palestine which are desert."

A communication was then read from Mr. Trelawney Saunders (who has added to that debt which English geographers are under to him by laying out the water basins, &c., on the well-known map of Palestine published by the Palestine Exploration Fund). After referring to "Mr. Reginald Stuart Poole's article in Dr. W. Smith's *Dictionary of the Bible* (3 vols., 1863), in which the then known identifications are carefully considered, and the bearing of Shishak's reign on Egyptian and Biblical Chronology is carefully elucidated," he added, "it may be worth attention that No. 58 Zaloumim is No. 57 in Poole's table, and the tabular arrangements of the latter seems to suggest its probable accuracy. Dr. Maspero's suggestion of Gath in reference to No. 55 is quite exciting, and is an instance of the prizes that the investigation of Sheshonq's list still has in store for inquirers."

Captain F. PETRIE, F.G.S.—I cannot help alluding to the debt



Bible students owe to Professor Maspero for his labours. His especial endeavour in this paper has been, to "rigorously transcribe" the Egyptian letters into their Hebrew equivalents, and thus avoid, as far as possible, all errors and chance of hasty assumptions, so as to obtain the exact names of the places to be identified. The success attending his efforts is acknowledged, and one instance of their value was recently pointed out by that well-known member of this Institute, the late Canon Liddon, who considered that M. Maspero's investigations as regards the list of Thothmes III (see Transactions, vols. xx and xxii) afforded not only "indirect confirmations of the truth of the Bible narrative," but were "an important contribution to the great fabric of Ancient Egyptian history, to which we may look with increasing confidence for the means of showing how mistaken are certain theories which, *for purely or mainly subjective reasons*, would place the dates of the earliest books of Holy Scripture so late as to be inconsistent with belief in their general trustworthiness, to say nothing of their higher claims."

The CHAIRMAN.—We have a visitor here this evening, Mr. Frederick Bliss, who, in connection with Professor Flinders Petrie, has been carrying on the explorations at Tell-el-Hesy, on the borders of Philistia.

Mr. F. J. BLISS, M.A.—I cannot help noticing the omission of Lachish from the list given by M. Maspero. It would have been satisfactory if something final and definite (I mean after the work at Tell-el-Hesy) had been added with regard to identification; of course unless you find an absolute inscription on a building, that is known to be of ancient date, and which can clearly give the name of the place, you have not arrived at a perfect and complete identification. Major Conder suggests that Tell-el-Hesy is probably Lachish, and Dr. Petrie, by his work, has made it seem more sure, and my discovery of the tablet at Tell-el-Hesy has been thought to help it on rather more still—though I am not quite sure, the only mention on the tablet being of Lachish (which has been connected with Tell-el-Hesy); and of course the finding of an inscription a thousand years hence in some town mentioning the Lord Mayor of London, would not prove that place to be London. Yet I feel pretty sure that Tell-el-Hesy is Lachish; but between the highest probability and certainty there is always a loop-hole.

The Rev. W. BAILEY, M.A.—I feel a deep interest in all that has been read this evening, for it so happens that during twenty years' residence in Jerusalem I know almost all the places mentioned, and it is only now, after many years' absence, that I have again returned from Palestine, where I lived in the midst of the places mentioned in the paper. I was with Captain (now Sir Charles) Warren, round about the Jordan, and, since then, with the discoverer of the Moabite stone, and I could not help thinking how in the present day so much is found to make us feel and realise the truth of the word of God. I have felt when a man has travelled in Jerusalem he will either go away a worse man or a better—he will have his faith confirmed, or he will go away with his mind set against God. You cannot go a step in Palestine without seeing that the word of God is true.

Mr. W. St. BOSCAWEN (F.R.Hist.Soc.).—I think Professor Maspero has done great service to Biblical geography in bringing together the monuments and the evidence of those monuments, and saying "see how these two fit."

The CHAIRMAN.—The hour being now late I will only mention, as regards the names in the paper, that finding but few references to their places in the Old Testament, and thinking that it might be of advantage to many to have the references given to those that Major Conder and others considered to be important,—I have drawn out a list, which may save trouble in future; I may mention that there are at least eighteen—perhaps more—places named in the Old Testament, particularly in the book of Joshua, the identification of which are already beyond doubt.

*Old Testament References to the Names.*

No.

11. Gaza. Judg. 16; Jer. 47; Amos 1, 6; Zeph. 2, 4; Zech. 9, 5.
12. Mageddo. Josh. 12, 21; 17, 11; Judg. 1, 27; 5, 19.
13. Rabbati (Rabbiti). Josh. 19, 20.
14. Taânaqou (Taanach). Josh. 12, 21.
15. Shaunama (Shunem). Josh. 19, 18; 1 Sam. 28, 4; 2 Kings 4, 8.
16. Bit-Shaïnla (Beth Shean?). Josh. 17, 11; 1 Sam. 13, 10.
17. Rouhaïba. (Rehab of the *Onomasticon*.)
18. Hapourama (Haphraïm). Josh. 19, 19.

19. Adoulmim. Bible ref. doubtful.  
 21. Shaouadi (Shadid, or Sarid ?). Josh. 19, 10.  
 22. Mahanema (Mahanaim ?). Gen. 32, 2 ; 2 Sam. 2, 8 ; 17, 24.  
 24. Bit-haouaroun (Beth-Horon). Josh. 10, 10 ; 16, 3.  
 25. Qadoutim. Bible ref. doubtful.  
 26. Aiaouloun (Aijalon). Josh. 19, 42 ; 21, 24.  
 27. Makidau (Makkedah). Josh. 10, 10 ; 10, 16.  
 28. Adirou or Adilou (Ataroth). Num. 32, 3.  
 29. Iaoudhamalouk (Jehud). Josh. 19, 45.  
 37. Qaqali (Kêilah). Josh. 15, 44.  
 38. Shaouka (Shocho). 2 Chron. 11, 7 ; 28, 18.  
 65. Aazama (Azem). Josh. 15, 29 ; 19, 3 ; (or Azmon) Num.  
     34, 4.  
 69. Fit-iaousha (Joshah). 1 Chron. 4, 34.  
 79. Adidima (Adadah ?). Josh. 15, 22.  
 99. Hananii (Anim ?). Josh. 15, 50.  
 100. Adorau (Adoraim). 2 Chron. 11, 9.  
 106. Diouati (Juttah). Josh. 15, 55.  
 108-110. Arada (Arad).  
 120. Baruk (Berachah). 1 Chron. 12, 3.  
 125. Shalhatou (Shalatin). Josh. 15, 32.  
 133. Iaourishalama (Jerusalem).

The meeting then adjourned.

#### COMMUNICATION FROM THE AUTHOR.

M. Maspero writes:—

“ Paris, Avril 2, 1894.

“ L'identification de Tell-Hesy avec une ville égyptienne dépend avant tout de son identification avec une cité hébraïque. Si le site moderne répond bien à Lakhish je ne trouve rien dans la liste de Sheshonq qui puisse être comparé à Lakhish et par suite à Tell-Hesy ; s'il répond à quelque autre ville de Juda peut-être le nom de cette autre ville se rencontrera-t-il sur la muraille de Karnak. De toute façon la recherche du nom sémitique doit précéder celle du nom égyptien.”

## ORDINARY MEETING.\*

THE PRESIDENT, SIR G. G. STOKES, BART., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

LIFE MEMBER :—The Hon. L. Kinnaird.

MEMBERS :—J. H. Anderson, Esq., Middlesex ; Miss F. Reade, India.

COR. MEMBER :—Professor Fritz Hommel, Ph.D., Bavaria.

LIFE ASSOCIATES :—Rev. L. P. Conrad, B.D., M.S., United States ; A. E. Martineau, Esq., India.

ASSOCIATES :—Admiral H. McC. Alexander, R.N., Ireland ; His Excellency Grant Bey, M.D., Egypt ; Rev. J. D. Bryan, Alexandria ; Rev. T. Stirling Berry, D.D., Ireland ; Rev. Principal E. Elmer Harding, M.A., St. Aidan's Coll. ; W. Kirkaldy, Esq., Surrey ; W. A. Lea, Esq., B.A., B.Sc., Canada ; G. Munt, Esq., Surrey ; Rev. J. Matthews, M.A., Middlesex ; Rev. W. H. Macpherson, United States ; Rev. J. T. Pettee, A.M., United States ; Rev. Prof. L. B. Paton, A.M., United States ; G. J. Smith, Esq., J.P., Middlesex ; Rev. C. H. Stileman, Surrey ; Rev. A. Sloman, M.A., Cheshire ; Rev. R. Towers, Middlesex ; Rev. C. C. Waller, A.B., Canada ; Rev. S. M. Zweemer, M.A., Persia.

The following paper was then read by the author :—

*AN ENQUIRY INTO THE FORMATION OF HABIT IN MAN.* By ALFRED T. SCHOFIELD, Esq., M.D., M.R.C.S., &c., Chairman of Council Parents' National Educational Union.

I HAVE written this paper in some haste during great professional pressure, in answer to a request conveyed to me by the Hon. Secretary to this Society; and I feel some apology is needed, not only for its imperfections, but for the selection of a subject that is certain to raise many more questions than it answers. And my apology is this: first, that the subject is continually more or less before me in connection with the education of children on the plan advocated by the Parents' National Educational Union, with which I am connected; and secondly, because, as will be seen, no question is of greater importance in the formation of character, or has a greater bearing on the advancement of the race.

The subject is one involved in difficulty, lying as it does on the borderland of the unknown, and touching the great questions of mind and brain action. I must therefore be excused if I am found expressing the movements of the one in terms applicable to the other, or in any other way using words coined for matter with reference to that which is immaterial.

\* 1st of 1894 Session.

I would also finally ask the forbearance of any physiologists that may be present, if in order to make the subject clear to those who have not pursued these studies, I preface my remarks with a brief summary of the general arrangement and structure of nerve tissues.

*Brain structure generally.*—The adult brain in man weighs between 46 and 53 ounces, with extreme limits from 23 ounces in an idiot to 65 ounces in men of the highest attainments. In women brains weigh between 41 and 47 ounces. The brain of the highest anthropoid ape weighs about 15 ounces. The brain is in two halves, right and left, and in four portions: the hemispheres and cortex, the seat of purely intelligent and voluntary actions; the middle brain, consisting of large ganglia, whence proceed the ordinary movements of animal life, *not necessarily voluntary*; behind this the cerebellum, or little brain, co-ordinating the movements, especially those connected with the erect position; and below, the medulla, which contains nearly all the involuntary centres connected with the maintenance of passive physical life.

The cortex, which increases in size in animals in proportion to the rest of the brain, in the ratio that intelligence supersedes instinct, is covered or rather composed of convolutions which by their number and depth afford a very fair idea of the amount of intellectual development. They increase steadily in intricacy as we ascend the animal scale; they also increase in man up to fifty years of age, after which they get gradually less marked; the brain as a whole also decreasing in weight about one ounce every ten years. The brain and spinal cord are each pierced with a continuous central tube surrounded with grey or nerve cell matter, which in its turn is surrounded by white or nerve fibre matter. The cortex or surface of the brain is covered to the depth of about a quarter of an inch with another layer of grey cell matter, the superficial extent of which is obviously greatly increased by the convolutions.

The brain is continually wasting and being repaired, the new tissue always accurately reproducing all the features of the old, whether these be congenital or acquired.

*Nerve structure generally.*—The nerve centres consist of three main elements; nerve cells, nerve fibres, and the groundwork or webwork in which both are embedded, called neuroglia. In the cortex this substance looks like ground glass, and under a very high power is seen to be traversed in every direction with very fine white fibres less than  $\frac{1}{10000}$  of an

inch in diameter. The nerve cells seem to be the starting point, and the centres of nutrition for the nerve fibres. The nutrition of the attached fibres is indeed a more obvious part of their work than the projection of impulses, which was formerly thought to be their main function. Any fibre cut off from its nutrient cell soon wastes away. In early childhood the cells are of a spherical, fusiform, or pyramidal form with few or no interlacing nerve fibres. Nerve impulses, starting in infancy and increasing in numbers and complexity till adult life is reached, are believed to form intercommunicating nerve fibres between the cells in every direction, until in manhood though there are still left many unbranched cells, the greater number have fibres given off in every direction. In old age again a good many of them appear to be broken off and the cells blunted.

*Blood supply.*—The grey matter containing cells is, to a limited extent, analogous to an electric battery, of which the wires are the nerve fibres. The vitality of these nerve structures is maintained by a constant supply of fresh arterial blood. By this means when the battery has discharged its nerve force, it is speedily recharged, and as this occurs most often in the grey matter, there is about five times as much blood circulating there as in the white or fibre matter. The great proportion of blood used by the brain compared with the rest of the body is certainly remarkable. While the brain is only about  $\frac{1}{15}$  part the weight of the body, the supply of blood is about one-eighth of the whole of that required by the rest of the body. The system of circulation is arranged so as to ensure the most constant and rapid change. The interdependence of mind and body is nowhere more clearly seen than in the question of blood supply. If it be suddenly *cut off* from any part, that part can no longer be used voluntarily; if the blood be *deficient* in quantity the thoughts often get confused and senseless; if it be *defective* in quality the very disposition seems changed, and the person gets gloomy and morose; if the *temperature* gets raised, delirium sets in; if *effusion* takes place, and the blood presses on the brain consciousness is lost altogether in an apoplectic fit.

*Ordinary functions of brain.*—The brain has already been divided into four parts, and these correspond to its leading functions. The cerebrum is thus divided into upper, middle, and lower regions; or cortex, mid-brain, and medulla. The first is the seat of intellectual life, or the sphere of the activities

of the *spirit* of man; the lower, of the necessary vital functions that carry on and store life's forces—the vegetative side of our life, or *body*; while the middle region is that of the functions of animal life, or what is sometimes called the *soul*. The actions connected with the cortex are voluntary, those connected with the medulla are involuntary or reflex, while those between the two partake of both varieties of action, being at first largely voluntary in character, but becoming more and more automatic in reflex as *habits* are formed. The difference of these four divisions of the brain is well shown in drunkenness. The upper region is affected first, and noisy manifestations of animal life are displayed unrulèd by the spirit. If the man be drunk, the middle region and the cerebellum are paralyzed, and all equilibrium and movements of animal life are lost. If the man is dead drunk, the medulla alone remains active, carrying on the functions of passive bodily life.

That the hemispheres or upper regions of the brain, and particularly the surface or cortex proper, are the centres for intelligent brain work, is proved by direct experiment, as we shall see when we consider the various actions of the brain. But we may here remark that the frontal region is supposed to be specially connected in some way with thoughts and ideas that do not result in bodily activity; the occipital and part of the parietal regions are the centres of sensation or perception, while the intervening portion is the centre for all motor impulses, which can be readily aroused by touching the part with electric stimuli.

In idiots the frontal region is found to be very deficient, while in intelligent men it is greatly developed.

Destruction of the sensory area in the *cortex* appears still to leave the *mechanism* of sensation (a dog will see, hear, and even feel, in a sense), but the *perception* is lost (it does not know what it sees, hears, and feels).

In the middle or motor area, districts have been carefully mapped out in the right and left hemispheres, corresponding with movements in various parts on the opposite side of the body; but it has been specially observed by Foster that the size of these districts does not correspond with the size of the part moved or the number of muscles or nerves it may contain, but to the more or less elaborate and complicated and intelligent use of the part. Thus the district for the arm is enormous compared to the leg; that for the thumb large as compared with the fingers. Another proof

that the nerve fibres increase according to the complexity rather than the number of movements is found in the fact that although the number of movements of the leg must be as numerous in a dog, or an ape, as in man, the pyramidal tract in the spinal cord by which they are conveyed is twice as large in man as in the monkey, and ten times as large as in the dog.

The functions of the brain develop in a fixed order, and Sir J. C. Browne has called attention to the fact that if this natural order is disregarded in education, the result is imperfect, and the mind is never fully developed. The various senses, the motions, emotions, and intellect all come to maturity at different times.

With regard to movements, those of mastication precede those of the foot and leg, then come the hand and arm, then the proper use of the tongue and lips, later on the power of speech and writing.

Imperfectly developed motor centres produce various imperfections in the execution of the movements involved, such as stammering, twitching, an imperfect gait, &c. One point of importance remains to be noticed. The brain centres are developed by exercise of the parts they govern, and whenever fully developed, the result remains. Thus if a limb be atrophied or useless from birth, it is found that the district in the cortex remains undeveloped; but on the other hand, if the centre be once fully developed by use, and the limb subsequently lost, it is found that although the lower centre in the spinal cord may waste, the higher centre in the cortex remains perfect, being probably maintained by its inter-communication with other parts. The bearing of this on physical education is obvious.

*Nerve currents.*—The more the brain is investigated the more does its broad description as a sensori-motor mechanism appear true. If we except a certain frontal area, and even this is doubtful, it appears that apart from the hemispheres and cortex, the nerve paths in the lower parts of the brain consist of sensori-motor arcs, the nerve currents arriving at the hinder part of the brain by the posterior part of the cord, and leaving the anterior ganglia, notably the corpus striatum, and descending down the front of the spinal cord in the resulting motor impulse. To use now the words of Dr. Hill, in his paper on reflex action, read here a short time since: "On these arcs, which collectively make up the lower system, are superadded arcs, the loops of which lie in the higher grey



matter. At the same time, therefore, that an impulse flows across the spinal cord, as a simple direct reflex action, a certain part of this impulse is also diverted to the brain along fibres which ascend in the outer part of the spinal cord; and from the brain descending fibres carry the impulse back again to the lower arc. Accurate measurements of the time taken by impulses in travelling through the grey matter have done much to throw light upon the route they follow; but we do not yet know whether we ought to speak of the conversion of a sensory into a motor impulse, as its passage through the lower network under the direction of nerve currents which originate in the higher; or whether the impulse when it reaches the lower grey matter takes in some cases a direct cross path, while in others it makes its transit through a longer loop. One thing is quite certain, namely, that the routes which are most frequently used are the most open, and therefore the most easily traversed."

The functions of the nerve-cells are various and must be considered in detail; the molecules, or particles, of which a nerve-cell is built up, are in such an unstable condition that any stimulus readily excites them to change; this molecular change is believed to constitute a nerve-cell action; it may be of very various degrees of violence; it may exhaust the nerve-cell in proportion to its violence (and when exhausted the cell cannot act again until restored by nutrition from the blood); it may affect the substance of the cell, and especially of young growing cells, so as to leave an impression on the cell, permanent in proportion to the violence of the action and the number of its repetitions. When a nerve-cell acts (whatever this may mean), impulses tend to pass off from it along its various connecting fibres; the force and number of these impulses depends on the violence of the cell action; if this is gentle there may be only a slight impulse passing off through the largest connecting fibre (the freest channel); if the action is violent it may overflow through the various connecting fibres in impulses increasing in force and number with the violence of the cell action.

If the foot of a sleeping (or deeply thinking) person is tickled it is quietly withdrawn; that is to say, the gentle skin irritation sends a gentle impulse to the sensory cells, which are gently excited, and send gentle impulses to a few motor cells; but if the foot be suddenly burnt, the sensory cell action, excited by the powerful impulse from the severely irritated skin, will be so violent that it will overflow through many more connecting fibres, and almost every muscle in the body

may be thrown into violent action, causing the person to spring vigorously away from the injury.

When we speak of higher loops ascending to the cortex, and when we remember that besides these loops the brain cells send off masses of fibres that ascend to the cortex and appear to end there, and when we ask what are the sources of the impulses that control these loops and fibres that are evidently the vehicle of voluntary actions, we are brought face to face with two great questions: "Is there a mind apart from the brain?" and "Can mind act on matter: or that which is immaterial on that which is material?" This subject cannot be wholly passed by, and must be here briefly touched on.

With regard to the second question Professor Clifford settles the whole point for us by the dogmatic statement that "To say, will, influences matter is neither true nor untrue, but simply nonsense." If this *ex cathedrâ* statement be true, I fear a good many of us talk great nonsense, and some of us will certainly do so to-night. Before answering it, however, let us consider our first question, as to the existence of mind apart from brain.

The existence of the will, which is the supreme assertion of mind, is proved by knowledge and experience. The formulæ, "*Cogito, ergo sum*," and "I know, I am, I can, I will," both express this. Feeling and thought and will are the only things we know to be real; all else is ascertained by our senses. The consciousness of effort as well as purpose in will is strong proof of its real existence. The contrary belief, that we are actually automatic, that voluntary actions are only so called because their automatic nature has not as yet been discovered, and that the mental phenomena that follow brain actions and movements, such as sensations of pleasure and so forth, are merely the products of such movements, or at any rate associates of them, as the melody is the result of playing on a harp, or motion the result of rowing in a boat, is negatived not only by experience but by the following considerations. Are we, for instance, as Dr. Courtney asks, "to consider that mental states are merely the products of movements of material molecules?" Is thought a secretion of the brain, or are we, in the words of Mr. S. H. Lewis and others, to speak of the equivalence and identity of mind and matter, so that thought and nerve action are two sides of the same thing, or to use one of the most recent similies, "that the mental and physical sensation

correspond as the convex and concave surfaces of a hollow sphere”?

The answer to all this in the first place (but by many this will be considered of no weight), is that such an idea is subversive of all moral principle.

In the next place we *have* the power of choice, selection, memory, and attention, all of which, when carefully analyzed and considered, have no correspondence with any form of nerve action.

Consider the faculty of attention. If all mental conditions (to quote Dr. Courtney again) were simply the material result or effect of molecular agitation of the nerves, it is difficult to say why some forms of nervous agitation should produce “attention,” while other forms exactly similar should fail to get themselves registered within the brain. We are looking upon some landscape; we attend to some features in this landscape; we notice some particular tree or figure, or colour, not always because it is striking, but for some capricious fancy of ours. How can this be if there be not a mind within us with laws of its own, which has a nervous mechanism, but is not the slave or result of that mechanism? The Greeks rightly decided long since that the mind was not the music of the harp or the motion of the boat, but the player and the rower.

A great attempt has been made to prove that all actions are sensori-motor reflexes, that all organisms are merely mechanisms; but although we act often on impulse, we are equally conscious of acting against it, and of the mind conquering all the sensory solicitations of the body, and refusing to transmit the natural motor impulse that would have resulted had we had no will. The brain is certainly most carefully isolated from all external impressions—in a bony case, floating in fluid, wrapped in membranes—except those conveyed by the blood and nerve currents; and yet these totally fail to account for actions contrary to these currents, and we must superadd therefore, that it is acted on by mind.

The action of an automaton, moreover, is characterized by ease, that of mind by distinct effort, and the mental fatigue is never in proportion to the amount of work done, but as to how far that work is reflex, or automatic, or voluntary.

Again, if half the cerebrum is lost, half the powers of the body go, and yet the mind remains as a whole. Moreover the brain tissue is incessantly changing, and yet through all

our life we preserve the consciousness of the same personality. This cannot be through the medium of the body, which is not the same, but must be through an independent mind. The mind does not produce physical energy, but it guides and directs it, like a man on a horse. Dr. Carpenter says, "The influence of a great idea, conceived by a thinker in his closet in controlling the action of an entire nation, is utterly disproportionate to any conceivable play of molecular forces that can be exerted by the physical agency of the thinker putting his idea into speech or writing." There may be automatic thinkers, in whom the will is absent or undeveloped, but though the dominant power is absent, even such have mind as well as body. The existence of mind therefore and the freedom of the will may be said to be axiomatic truths.

And now to return to Dr. Clifford as to the relation of this mind with matter. Professor Ladd, in his *Physiological Psychology*, says: "The human brain is a vast collection of material molecules, whose constitution and arrangement is such as to connect them with certain forms of external physical energy.

"But they are also capable of standing in a yet more surprising and unique relation to a being of a different nature from their own, *i.e.*, the mind. These latter relations involve a causal connection, as truly as do the relations of the natural physical forces. That material molecules and a being of the kind called mind can be causally connected is indeed a mysterious fact; but because of its mystery it is not less to be acknowledged as a fact. The assumption that the mind is a real being which can be acted on by the brain, and which can act on the body through the brain, is the only one compatible with all facts of experience."

Neuroses, or nerve actions, produce psychoses, or mind actions; thus a prick produces pain. The light on the eye is a physical action, the impression on the sight centre a physiological one, the perception of it a psychical one.

The ordinary condition of the nervous system is that of a moderately charged battery that can be discharged by the completion of the circuit and re-charged by the blood. The will can complete this charged circuit. Mental causes can, as we have said, produce physical effects, and physical causes can produce mental effects. "We have every reason to believe," says Professor Bain, "that with all our mental processes there is an unbroken natural succession."

We must notice however, carefully, as to automatic

actions, that what we have power to will is not the action of certain muscles or nerves, but effects or results. The automatic machinery is all there; our will puts it in motion. The word voluntary muscle is therefore to a certain extent a misnomer, as few are under the direct control of the will. We cannot will the method but the result.

*Actions classified.*—Having then admitted that action may be originated in the body by a purely mental impulse called the will, we are now prepared to classify roughly the entire range of actions from the lowest to the highest in both body and mind. They are as follows:—

(a) Pure natural physical reflexes of three varieties:—

1. *Unconscious excito-motor actions*, generally called automatic because the exciting agency has not been discovered, such as the regulation of the size of the capillary blood-vessels, of which we are unconscious.
2. *Conscious excito-motor actions*, as the acceleration of the beat of the heart, producing palpitation, of which we are conscious.
3. *Sensori-motor actions*, such as laughing when tickled, when we are conscious of the causal sensation.

(b) Mixed physical reflexes, which are of three varieties:—

1. *Mixed sensori-motors and voluntary actions*, such as breathing, which, though generally reflex, can be controlled to a large extent by the will through the cortex.
2. *Deferred natural reflex actions*, such as the erect position, which is apparently learned artificially, but is really a reflex action not seen at birth, but of deferred development.
3. *Instinctive habits*. These are combinations of simple reflex actions for definite purposes, but without need of intelligence. They are best noticed in animals, as in the flying of birds. Pigeons can fly after the removal of the cortex. Frogs, when deprived of the cortex, can balance themselves on a board slowly turned round, and will croak when stroked, but never move voluntarily. At the same time if all the brain is taken away they can only execute simple reflex movements with their limbs. These

experiments show respectively the seats of reflex action (the cord), of complicated automatic action (the lower brain), and of voluntary acts (the cortex). It has been said by Romanes that instinct is partly due to lapsed intelligence. It may have come out of confirmed habits, and in this case mind must precede instinct and not succeed it. This carries mind a long way down the scale, and prepares us for Professor Ladd's remark that "automatism belongs to all living protoplasm." It is for this reason it is said, "an amoeba has a will of its own."

Instinct appears to culminate in the articulates, such as ants and bees, while intelligent action culminates in the vertebrata, as man. The former are like barrel organs, and can only play certain fixed tunes, however complicated, while the latter are like organs that can produce any melody at the will of the player.

(c) We come next to *psycho-physical acts*, mixtures of mind and brain. These are:—

1. Artificial, or acquired reflexes or habits; these originating in the will became automatic by use, and are the chief subject of this paper.
2. Voluntary actions acting *with* physical impulses.
3. Voluntary actions acting *against* physical impulses.

(d) Lastly, we reach *actions purely psychical*, which we will simply enumerate:—*Reflex* ideas, desires, emotions, and perceptions produced by the mind without the will.

*Artificial reflex* thoughts started by the will, continued by association; and lastly, purely *voluntary* ideas and emotions.

Before now passing on to enquire into the nature of habit, let us pause for one moment to consider the wisdom displayed by fortuitous evolution (if such, indeed, be our origin) in the great fact that all the processes in our body are of a reflex or automatic nature that are connected with the mechanism of life, and are not subject to the control of our will, but proceed in a large measure even without our consciousness; while on the other hand all the actions of physical life or the expenditure of animal force is placed in

the direct control of our will; so that while we have little or no share in the accumulation of our life capital, we have a large control over its expenditure. I do not say "entire," because some is used in carrying on the natural functions of the body. Were the fact otherwise, and our will had to control the processes of physical life, life would indeed not be worth living, and intelligent existence an impossibility. The voluntary and non-voluntary systems form, as a whole, two well-marked centres of government, each having at its command the necessary nerves, muscles, and organs. In the former case the nerves are white and the muscles striped, in the latter the nerves are mainly grey or non-medullated, and the muscles plain or smooth.

What we have now to consider is how, in the evolution of higher intellectual life, we have the power at will to change voluntary into involuntary action, to an almost unlimited extent, by the formation of habits; a process important to be understood, and of the greatest bearing on the well-being and progress of the race?

*What habit is.*—Having therefore now briefly touched on a few of the leading points connected with the ordinary action of the nervous system, we proceed to consider the direct subject of this Paper, "the formation of habit in man." Let us first of all see what we mean and understand by "habit."

It is difficult to conceive of habit with reference to inanimate objects, and the word is no doubt to some extent inapplicable, and yet it is an interesting question as to what are the limits of its sphere of action.

Are the very laws of motion the result originally of habit? Are the chemical combinations of elements and the formation of different constant natural compounds and mixtures the result originally of long repeated repetition forming at last habits with cast-iron bonds that cannot be broken? Again, do we not see in an old dress, even in a room, a something that speaks of habit, an adaptability of shape and crease from constant wearing and use, or of fittings and furniture, that cannot be seen in a new coat or in lodgings? Does not an old violin that has been the property of some great master (not only made by some great maker) retain in its very fibres the habit of resounding to the grand chords he struck with far greater ease than any instrument that had not acquired this "habit" by long use? Passing on to living things. Do not trees acquire habits of growth from their environment,

and in the lower forms of animal life does not this open up the whole of the great question of the formation of natural reflexes or automatic action and instinct? Are the rhythmic pulsations of the jelly-fish or the movement of an amoeba the outcome of purely reflex action, or were they at first voluntarily acquired habits passing by long use into hereditary reflexes?

In the marvellous labours of the ant and bee instinct seems to have reached its apogee. Do they, as Romanes suggests, speak to us of a lapsed intelligence that having by long use formed all needed habits, has ceased to act when these have been crystallized into instincts? These questions, fascinating and interesting though they may be, are unanswerable in our present state of knowledge. Although the evidence in favour of lapsed intelligence increases, Prof. W. H. Thompson from the Chair of Physiology in Belfast, read only last week (Jan. 1894), "that the amoeba presented active and spontaneous movements, and that here one not only meets with a power of choice, but also an intelligent consciousness in selecting food."

Habit in man, as generally understood, means an act or thought, or sensation, or any combination of these, simple or complicated, that has been sufficiently often repeated to no longer require the same intelligence and will-power for its execution that were at first needed. It thus becomes an acquired or an artificial reflex.

Nearly all natural instincts in animals have thus to be formed as artificial reflexes in man. In man artificial habits formed at will replace instincts of a fixed character, or, if you please, voluntary habits replace automatic habits. Routine is living by habit. We sow acts and we reap habits; we sow habits and we reap character; we sow character and we reap destiny. Habit has well been called the railroad of character. Habit is physical memory. Memory is psychical habit. Character is organized habit. It is wonderful to note that even fixed habits that have passed (as we have suggested) long since into instincts or reflexes, can be modified by environment. It is the habit of all ova to build organisms in accordance with certain exact laws. But the ovum of a working bee that would produce a working bee is made to produce a queen bee by altering its food and feeding it on royal bee bread.

*The force of habit.*—The force of habit is, however, very great, and is only short of natural reflexes, which are



omnipotent in the body. No power of mind or will can stop the beating of the heart or the movement of the stomach, and a habit may be so formed as to be almost as difficult to check. Darwin found he had acquired in common with most men the habit of starting back at the sudden approach of danger, and no amount of will-power could enable him to keep his face pressed against the plate glass front of the cage of the cobra in the Zoo while it struck at him, even though he exerted the full force of his will, and his reason told him there was no danger.

The Duke of Wellington is credited with the dictum that habit is as strong as ten natures, and certainly to see what a soldier will do and is worth in a campaign when seasoned and well drilled, compared with a raw recruit, one feels that this statement is under rather than over the mark; for he owes all his value to "habit"! If an established habit is broken by the will the lower centres rise up in rebellion, so accustomed are they to the easy yoke of that which has been often repeated, that the effort of control required, as in the process of breaking a habit, over lower physical centres, often extremely painful.

*Physiology of habit. How formed.*—Referring to the description of the brain in childhood it will be remembered that it is something like a wide common over which are traces of many ancient tracks but no fresh paths. Habit strikes out fresh paths if the result of education, or re-forms old ones if they are the outcome of heredity. In all cases of true artificial reflexes or habits the will is the starting point, and a purely voluntary action takes place. This is repeated continually until, as C. Bastian and others believe, not only is a well defined brain path established between the arbitrarily associated groups of cells, but this path is physiologically present in the brain in the form of nerve threads or fibres; or in the graphic language of Dr. Michael Foster: "The will, blundering at first in the maze of the nervous network, gradually establishes easy paths. When once this is effected the slightest impulse seems to start the nerve current along the whole of the associated groups and produce the habitual action. The nerve current follows this route not now because it is guided by intelligence, but because this route offers the least resistance from habitual use."

There are one or two interesting points in the formation of a habit.

In the first place the action must never be varied even

for a day. If it be the learning of some steps in dancing they should never be changed till fixed in the brain. Again, it is of great importance, and this has a very wide application to the training of children, that the habit be taught and executed accurately. If the steps are taught in a slovenly way they will always be executed in a slovenly manner.

If a child learns sometimes that two and two make five, and at other times that they make four, there will always be confusion in the mind or brain paths as the case may be.

Again, there is a great tendency in the young for all repeated acts to become fixed habits, as in making grimaces, or the use of slang words.

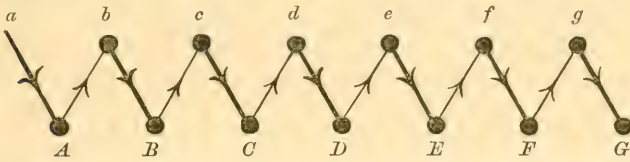
In those whose intellect is deficient this is far more marked. All such cases are creatures of strong habit and routine, and they like everything done at the same time each day. Miss Martineau tells us of an idiot who required any new thing done to be repeated at the same hour each subsequent day. His hands were washed and nails cut at 11.10 one morning, and next morning at exactly the same hour he came to have it done again, and yet he had no knowledge of time, and could not tell it on a clock. There must have been some very accurate unconscious cerebral process that told him when the twenty-four hours had elapsed. If seven sweets were given him one day, he would take neither six nor eight the next.

Again, fresh nerve paths tend to consolidate apart from actual repetition. A new task learned in the evening becomes easier to perform each morning than it was the night before, and easier still on Monday morning than it was on Saturday evening. The Germans go so far as to say that we learn to skate in summer and to swim in winter. What is exactly meant is that having been taught skating one winter, we go on learning it unconsciously all through the summer, or that we begin much better next winter than we left off at the end of the preceding one.

Attention in the formation of the habit seems greatly to deepen its impression on the brain, and make it much more easy to establish. A good memory, which is a psychical habit, is thus established by attention.

*Results of formed habit.*—A formed habit of average complication produces a sort of reflex peristaltic nerve current between the associated groups of cells. Supposing it is a question of learning the clog dance and alternately tapping

the floor with the toe and heel in rapid succession, the process is somewhat like this—



The small letters being sensory, and the capitals motor centres connected by the nerve threads of habit. The will starts the nerve for this step by placing the toe on the ground by an impulse from *a* to *A*. Before the habit was formed this would be all, but now it is but the first link in a long connected chain, along which the nerve current passes with great rapidity. The moment the toe strikes the ground, the sensation is passed to the brain along *A b*, and this is reflected as a motor impulse to strike the heel along *b B*. This in its turn producing a sensation along *B c*, starts the motion of toe-striking along *c C*, and thus the motion continues till stopped at *G* by the fiat of the will.

Once a habit is well established on such lines as these, the interference of will or mind only spoils its perfect action. Whenever knitting has become automatic, if you think about the formation of each stitch, you have to knit much more slowly, and are more liable to make mistakes. A fixed habit is thus deranged by volition.

The more fixed a habit becomes, the less of the body is required to execute it, and thus a great economy of force is effected. In commencing piano-playing, the young performer plays with her hands, and arms, and body, and legs, and head, and often her tongue. As she forms a perfect artificial reflex, less and less of the body is moved, until at last it is literally nothing but the hands and wrists that are engaged, the brain being at perfect rest, or thinking of something else altogether. Habit is thus of great economic value.

Habit which is physical memory is of such importance to character that a spinal cord or brain without such memory is either idiotic or infantile. Artificial reflexes last long if well formed. In early life Robert Houdin, the conjurer, trained himself in the difficult habit of reading aloud while keeping four balls going in the air. He did not practice this for many years, and yet after thirty years

found he could still read and keep three balls going. Any one who tries this feat will understand its difficulty.

Artificial reflex habit resembles respiration, and still more coughing, in that these occur naturally by reflex action, but can be modified or stopped by the will.

Habits, in spite of Weissmann, formed during life strongly tend to become hereditary. This is clearly seen in the love of strong drink.

*When to form habits.*—The easiest and best time to form habits is in the growing structure in early childhood, notably before fifteen years of age. The earlier the period that habits are formed the more lasting are they, and reappear at a late period of life when other habits acquired since have passed away. Plasticity of brain is essential, that is, tissues weak enough to yield to influences, and yet strong enough to retain them. After the brain is fully developed, that is, after thirty, or perhaps later, to acquire new habits or to give up old becomes alike more difficult. In old age we find, as we have said, that those habits that are acquired last, are lost first. As a rule, personal habits are acquired before twenty, professional habits between twenty and thirty.

*Physical habits.*—Let us now consider a few leading habits, physical, mental, and moral.

1. Physical habits that modify natural reflexes. Thus, one may get accustomed by degrees to digest indigestible things, or to tolerate an excess of alcohol, or to blush very readily, or not at all, or to vomit at the sight of certain articles of food, and so on.

2. Or physical habits that are new products altogether; that is, real artificial reflexes. These are innumerable; they extend through all our being, are insensibly being formed whenever an act is repeated sufficiently often, and are often only detected when it is too late to alter them.

They are amazing in their intricacy and variety as well as in the extraordinary ease they give when once firmly established in the performance of the most difficult and at first impossible tasks.

The old saying, "It's nothing when you are used to it," or the couplet—

"If at first you don't succeed,  
Try, try, try again,"

simply mean, if a thing is too hard to do, establish a habit and you will accomplish it.

I would repeat here that in what we call voluntary actions all we do is to will a result, as of raising the hand to the mouth. The ease with which we do it and indeed the power to do it at all arises, not from our will-power being able to control the so-called voluntary muscles, but in their being already associated for the purpose by long established habit. Where no such habit exists an action becomes well nigh impossible, however strongly it may be willed. By long habit, hereditary in nature, we always swing our right arm with the movement of our left leg, and the left arm with the right leg. Let any one *will* the contrary, *i.e.*, to move the right arm with the right leg and *vice versa*, and however strong the effort of will may be, they will find in the end that it is powerless to overcome this established habit, except most awkwardly, and for the shortest time. The intense difficulty of the one movement and the perfect ease of the other, both in themselves equally easy, is most striking.

Let any one *will* to play the violin, or piano, or to skate, or swim, or in short to do anything that requires the formation of habits, and they will see it is impossible ; and that to do so at all a habit must necessarily be formed for the very purpose : and then behold ! the thing which was impossible before is executed with almost contemptuous ease. Few of us know what bundles of habits we are, and we imagine many of our actions to be voluntary which are really artificially automatic. Let any man over forty try to wash and dress himself in any but the accustomed order, and he will see what difficulties arise. He may not know the order in which he washes his face, but the hands know. He cannot tell which arm is put into the coat first, but the arms know. He cannot tell which foot is put into his stockings first, but the feet know. Before I begin to dress, from long habit I am almost compelled to pull up the blind a certain exact height, and if I fail to do so, I feel an inward impulse that is not satisfied till it is obeyed.

Consider the habit of shooting ; the perfect ease with which the trained sportsman, the moment the grouse rise, aims and fires well nigh automatically at the birds, who themselves have acquired *fin-de-siècle* habits (as Sir Joseph Fayrer told us) in learning to avoid the telegraph wires as they fly, which in earlier times they always struck against.

Look what an automaton a soldier becomes ; so that the very dinner he may be carrying, as Huxley tells us, is dropped

unconsciously into the gutter if he hears that magic word "Tenshun," which in his mind is so associated with his little finger and the seam of his trousers that his hands at once fall to their allotted place. But time would fail us to describe the marvels of physical habits, and we must pass on, especially as we have still greater wonders in store.

*Mental habits.*—Habits of thought are as truly and readily and often unconsciously established as habits of body, and indeed the two are sometimes inscrutably mixed; as in character as displayed in handwriting as well as in the lines that habit has traced upon the face, rendering physiognomy a true science. We have also ideal habits, and here as elsewhere habit means ease.

*Attention* may be deliberately manufactured as a habit by the inattentive. For this is the charm and value about habit; that if we begin soon enough, and particularly in childhood, and pre-eminently before the age of ten, we can absolutely engraft into the child's character many of those valuable mental qualities which it may lack. The habit of *enquiry* is easily acquired in young life, and is invaluable in after years, and simply means going through life with one's eyes open instead of shut.

The habit of *perfect execution* is invaluable, but must be taught early. Perhaps no other mental habit leads to greater success in every calling in life. Sloyd is the physical means by which this habit is best taught in childhood; for the essence of sloyd is not what is made, but that it should be perfectly finished in all its parts.

*Industry* is another invaluable habit.

*Moral habits.*—But we must pass on to moral habits. Now if we wish to produce some valuable moral quality in a child, the easiest way to do it is to establish the quality as a habit; the most difficult and uncertain is to depend on direct precept. To be always telling a child to be truthful is a poor way of making him so; but to accustom him to use his words in talking exactly as a painter uses his colours in painting, so that his word picture shall be a faithful copy of what he is describing, painted in words instead of water colours; this persevered in, will give him the habit of truthful speaking as a fine art, apart from its moral value, which of course will only strengthen the habit. In a similar way most moral qualities can be formed as mental habits—deliberately, surely, and easily, as compared with any other method; and if sufficiently well established, it is harder to depart from them than

to display them. Thus decision, self-control, obedience, self-respect, unselfishness, courtesy, reverence, can, one and all, be formed by frequent repetition in early life. We know nothing of the mind tracks that ensure their permanence; all we know is that they are as sure and lasting as physical habits.

In this connection those words of Holy Writ derive an added meaning: "Train up a child in the way that he should go, and when he is old he will not depart from it."

*Value of habit.*—And now in bringing these fragmentary remarks to a close, let me point out first the value of habits as a whole, and lastly their drawbacks; for they have drawbacks.

Habit is economical. It has been well described as using the interest of nerve energy instead of the principal. The absence of fixed habits is misery, and is the source of nearly all indecision of action and of character.

Habit alone enables things otherwise impossible to be accomplished, such as playing the flute, violin, or piano. But for habit we should spend a whole day in doing one or two things with great fatigue of mind and body, such as the continued effort to balance the body in the erect attitude by sheer force of will, or to read a book, or to walk.

Habit gives speed, accuracy, and ease. The will, as we have seen, can only set habits in motion, and is powerless to act when such do not exist. The unconscious ease of a well-formed habit has been well illustrated by fixing a wafer on a looking-glass, and while keeping the eyes fixed on it, moving the head in a circle. The eyes will be seen to be moving in every part of the orbit, but cannot otherwise be known to move at all: so unconscious and without effort is the action of the complicated muscles that move them, which by the way are all so-called voluntary muscles.

Habit forms character, or at least a good deal of it. Up to a certain point our character is formed *for* us by heredity, beyond this it is formed *by* us by habit. Skill is entirely the result of habit. To seek to be ambidextrous is folly. Specialism is everything in the body, and the habits that suit the right hand do not suit the left, nor the left the right. The left hand is just as awkward with a knife, as the right is with a fork. Some callings may require a certain measure of ambidexterity, but it is against all true development, and is common in idiots.

Habit adapts us to our environment, without which we should die. A bookbinder in a little den in Paternoster Row

is as happy and healthy as a farm labourer in the Midlands. Each has become adapted to his environment by habit. Let them change places, and the chances are both will die. Sir Charles Lyell tells us of some English greyhounds exported to South America for coursing hares on a raised plateau some 6,000 feet high. They were useless on account of the unaccustomed rarity of the air, but they produced pups who could course as well as the dogs of the country from a formed habit. Some habits are the offspring of necessity, others of caprice.

*Drawbacks of habit.*—But there is another side to habit that must be alluded to in conclusion, and that is its drawbacks. An illustration will explain this. In suburban dwellings, with a garden and locked gate in front, there is often an arrangement by which the gate can be opened from the house by pulling a handle that raises the gate latch. When the gate bell rings in the hall it is equivalent to a sensation reaching a conscious brain. The maid then comes and looks out to see who is there before she pulls the handle. If it is a person she wishes to admit, she pulls the handle which lifts up the gate latch. The maid is the mind which considers the impulse received by the brain, and does not send a motor impulse until the will determines what shall be done. This is a type of a pure voluntary action.

If, however, to save herself trouble, the girl fastens the wire that should ring the bell round a pulley in the hall to the wire that opens the gate, the result will be that when a man pulls the bell handle, he rings no bell but opens the gate by a reflex action. This is the formation of an artificial reflex, only it cannot be thus made at once by the will but must be gradually formed by frequent repetition. The advantages of the voluntary action were—the maid could admit whom she pleased, and none could enter without her knowledge and consent. The drawbacks were—it took her nearly all her time to answer the bell, and the man had always to wait for a time at the gate.

When the action is changed into a reflex one, the advantage is that the man is never kept waiting, for pulling the wire opens the gate, and the servant never has to answer the bell. The disadvantage is she no longer knows or can control who enters the garden. Habits thus may become our masters. There is a story of a lady engaged to play at a concert who took too much at supper, and the result was she



not only kept on playing too long, but whenever her fingers rested on the keys she started playing like an automatic musical box, and could not be stopped. Girls who drill holes in buttons in Birmingham are said during their dinner hour as they pass along the streets to be constantly continuing unconsciously the same movements with their fingers.

Habit is often used to excess with bad results. Hammer-palsy arises from incessantly using the hammer in making knives till the associated group of cells is worn out, and paralysis sets in; writers' cramp is another illustration.

Habits that have become unconscious may be put in action by using wrong stimuli. When dressing for dinner one frequently winds up one's watch by mistake, and some in changing their clothes have gone to bed unconsciously.

A bad habit is a terrible thing when thoroughly fixed. Swearing is a good example of this, and of the tenacity of a habit when firmly established. It is a drawback when processes that should be intellectual become mechanical by habit, as when prayer is said by rote and not prayed; it is this that constitutes all forms of "cant."

Habit blunts the feeling both as to right and wrong, and as to pleasure and pain, and when purely automatic abolishes it. A man may get such an inveterate habit of lying as to lose all sense of evil. So with other sins.

A person travelling or yachting takes great pleasure in it at first, but if he is ever doing this and gets into the habit of the thing, it loses its charm.

Games amuse when occasionally played, but when they are incessantly pursued, and an automatic habit is established, a large amount of the pleasure goes.

Habit may induce error, as when at the close of the year from long habit the same date is carried on into the next year, until the new habit overcomes the old.

Such then are some of the pros and cons of this important variety of brain action, and I must now leave the matter in your hands for discussion, asking in conclusion your forbearance if I have wearied you with details which some here know far better than myself; or if in using more popular language than is perhaps general in this learned atmosphere, I may have failed somewhat in preserving the high standard of preceding papers.

The PRESIDENT (Sir G. G. STOKES, Bart., F.R.S.).—I have, in the first instance, to return your thanks to Dr. Schofield for this most interesting and suggestive paper. (Applause.) I now invite those present to begin the discussion after the Honorary Secretary has read a communication.

Captain FRANCIS PETRIE, F.G.S.—Sir B. W. Richardson and Professor Burdon Sanderson have written regretting their inability to be present to-night. The following communication is from Dr. Alex. Hill, the Master of Downing.

“Oxford, Dec. 2nd, 1893.

“My dear Sir,

“I am much obliged to you for allowing me to see Dr. Schofield’s paper. It treats of one of the oldest of problems but one which will always be of infinite interest to the human mind. Are we free agents in selecting our actions, in forming habit and therefore in developing character, or are we instruments played upon by the forces of nature or by a Power Divine? Is the note we utter our own note or the sound evoked by circumstance or by the Deity? Unless it be our own we are puppets and equally irresponsible for harmony or discord whether the player be God or chance.

“We wish to believe that the ‘mind is the player’—nay, we can go further and say that it is good for us to believe it, and this mere statement is in itself a proof, for if our belief in our responsibility influences our actions it is clear that we *have* the power of directing them. Physiological proof is however out of the question; we can simply ‘admit (or presume) that action may be originated in the body by a purely mental impulse called the will.’ All attempts as proof are but illustrations.

“Different methods of stating the case appeal to different minds, but for myself I find that the nearest approach to a proof may be put very briefly in such form as this:—The energy received into the nervous system through the sensory nerves is redirected through motor channels into action. A condition termed consciousness accompanies or marks the passage of this force, but one cannot conceive of the consumption of force in the production of this state of consciousness. All the force received must be accounted for in chemical change within the nervous system or in outward movement. The condition of consciousness however presupposes the power of selecting action. Consciousness cannot exist without calling into existence the will, although the will may manifest itself in checking action only, not initiating it. But if

consciousness is not an exhibition of force neither is the will. It is extra-physical.

“In the paper which I had the honour of reading before the Institute (Vol. xxvi) I suggested a possible anatomical explanation of the formation of habit. It is at present but a hypothesis, and we know so little of the ultimate structure of the ‘ground substance’ of the nervous system that the hypothesis if not disproved is likely to remain for long unproved; but the highest magnifications seem to bear out the opinion that the ground substance is a network the strands of which are of almost infinite tenuity. It is possible that the passage of impulses increases the width or conductivity of these strands, beats down paths in fact along which subsequent impulses find it easier to travel.

“I am glad to find that Dr. Schofield believes in the inheritance of habit, for whether my anatomical explanation be correct or no, habit can only be explained as due to a physical change of some kind in the nerve-tissue, and if the habit be transmissible from parent to child, its transmission is due to the inheritance by the child of the alteration in the nerve-tissue acquired by its parent. We need no longer try to settle the much discussed question of whether acquired characters are transmissible by looking out for cases in which gross anatomical changes such as shoemaker’s chest or carpenter’s thumb are inherited by children not brought up to their parents’ trade, but we may assert with confidence that the central nervous system as modified by the deliberate choice of the individual tends to be transmitted to his offspring.”

A MEMBER.—May I ask the meaning of the word “sloyd” which occurs in the paper?

The AUTHOR.—Sloyd is a Swedish method of instructing children in habits of perfect execution. It was invented many years ago, and consists of a sort of carpentry and in making simple things, such as rulers and other things, neatly and accurately, according to a model given to the children. A child may spend a week in making a thing before he makes it of the exact size given; there are many classes for it now in England, formed with the view of teaching children habits of exact execution.

Dr. GERARD SMITH, M.R.C.S.E.—The question of the hereditary transmission of acquired habits seems to me most important in connection with this paper. Whether we believe, with Darwin, that acquired characteristics are transmitted to our offspring, or

with Weissmann, that habit forming simply brings out already existing tendencies in the individual, which are not transmissible, it appears to me that our responsibility is equally serious. In any case, we do know that habits are eminently contagious, if I may use the expression; and most of us will agree that in the case of some physiological modifications produced by acquired habit, it is most certainly true that the sins of the parents are visited upon the children.

Take the instance of intemperance, opportunities of studying which are only too easily obtained in professional work; if the habit has passed beyond the stage of a mere mental preference, and has become a physiological habit, actually impressed upon the brain by pathological changes, we must confess that it becomes transmissible, up to the time when the mental proclivity becomes a physiological change, the case is best treated by moral means, and after that point, by the physician.

The immense importance of this subject in the light of both physical and mental or moral education cannot be over-estimated. I have to do, in professional work, with the treatment by gymnastics of weakly and deformed children; and I could give ample evidence of the difficulty of eradicating physical habits formed in early childhood; even the habits of correct standing and breathing, being often things which have to be taught with difficulty, after eradicating incorrect habits.

In the physical education of children one sees the value of teaching co-ordinate movements, and the way in which such co-ordination is gradually attained is interesting in the light of the remarks of Dr. Schofield with regard to the formation of actual nerve paths and connections between various groups of cells.

REV. CANON R. B. GIRDLESTONE, M.A.—I should like to say one or two words on this interesting subject, though it is really so full of matter for thought, that it is rather difficult to concentrate one's mind on any one particular phase of it. On page 140 of the paper a question is raised which is certainly a most important one, viz.: Whether mind can exist apart from body. I suppose the writer of the paper means mind in man and not mind generally, for the phrase may include beings that are various. There are some that are pure spirit and have not a body at all, but the writer means, I presume, whether the mind can be imagined as separable from the body, and I cannot help thinking that although we may view the matter rather differently, yet that

all are perfectly of one mind on this matter—that really the body is the nursery of the mind; that the brain is the school of the mind, and that they must be together in this human nature at any rate, whatever may be the case in any other state of being.

In the present day much is said about automatic action and matters of habit, but when you examine an automatic machine you find it is simply condensed mind. There is all the mind there, only by mechanical processes the mind is reduced to a small compass and the automatic machine is the embodiment of the mind. I suppose if any human being or animal performs an automatic action, all we mean is that he does what he does unwittingly. The mind is there, only it is not the mind of the automatic agent, but somebody else's mind, and so in the case of an automaton of any kind; and the great question which concerns us is, whose is the mind in the universe that causes so many things to be done so uniformly without any apparent physical agency at all?

Then in regard to the question of instinct which has been so much discussed by Dr. Romanes and others, I see the idea is suggested on page 144 of the paper, "that instinct is partly due to lapsed intelligence." I remember a little book, by Isaac Taylor, *The World of Mind*, in which he says the distinction between human intelligence and instinct is that human intelligence is free reason, and instinct is fixed reason, and I think that fits in with what has been said here; but I think in the case of lapsed intelligence, too, you cannot tell how it begins.

I rather desiderate, through the paper, the use of the word *deliberate* instead of *voluntary*. I think there is a great distinction between things being done *with* your will and *by* your will. Deliberate action is a great deal more than voluntary action. At page 145 of the paper we read, towards the middle of it, "What we have now to consider is how, in the evolution of higher intellectual life, we have the power at will to change voluntary into involuntary action." I should prefer to say "change *deliberate* action into *automatic* action"—for it is the very deliberateness of the action which makes it superior to other actions which gradually become more and more automatic. On the same page the question is asked—"are the very laws of motion" (I suppose of the physical world generally) "the result, originally, of habit?" But, by the definition of habit given above, it cannot be so, for habit is said to be the result of

voluntary action; and I suppose nobody thinks that the first efforts in the direction, say of gravitation, were voluntary on the part of the molecules that performed those efforts; the voluntary action does not lie in the things but in the Person who arranged the things.

Then comes a question, which has been touched upon by another speaker, which is a most interesting one—viz., the question of transmission. Some of the cases mentioned in this paper relate to the animal world and not to the human world; and is not it the case that there is more of transmission, in proportion, amongst the lower animals than in the human race, owing to the fact that human personal responsibility is so much greater than that of any animal in the lower world?

In the human race there seems to be a marked distinction between physiological or physical transmission and mental or moral transmission. Take the case of the Chinese children's feet;—is not it strange that after all these centuries during which their feet have been pent up (and these little children suffer agonies for years, as a Chinese lady told me), yet that, after all, the foot is exactly the same when the child is born as it was in former days? It seems to show that physical habit is not changed in spite of all the misery and suffering that takes place.

Then in regard to mental and moral habits; it would be very convenient for school boys if they had transmitted to them the habit of learning. My own father, when he was over eighty years of age, could quote an ode from Horace with the greatest ease, and he was very vexed with me that I could not do it equally well; but I should be glad if I had an instinctive knowledge of Horace and many school boys would be delighted if they had, but it seems to me the more you get into the moral and mental state the more you see that transmission has to give way to mental action, and I should like to hear discussed whether a good habit is as easily transmitted as an evil one, for instance, temperance. In fact, it seems to be hardly a case of habit being transmitted, but, rather, a strong desire, for everybody's habit must surely be formed by the individual. There is one other point that I rather desiderated at the close. Are habits destructible? An inveterate habit—can it be done away with? I suppose we should say that whatever the will has done it can undo. But it sometimes happens, in such a matter as drink for instance, that not only has the habit been formed but a

craving has been created which is very strong, very maddening, and very violent, which is something over and above the original habit formed. Supposing, therefore, you have the power to undo the habit, you should also have the power to undo the craving which has grown out of the habit: this is a very interesting question which is partly physiological and partly ethical. The truth is that habit may be a great blessing or a great curse. Bondage, from a moral point of view, is habit, and it is the essence of Christianity to undo bondage; but how does it do it? By the expulsive power of new affection. It is not so much by playing on the cravings as by teaching and implanting a new craving, so to say, in human nature, which the psalmist put into words when he said, "create within me a new heart:" and after all, the wish is the thing that plays on the will, and the will acts upon it, and decides whether the wish shall be accomplished or not. When the new wish comes and the new moral desire begins to tell, then there is a counteracting force which is more effective than a mere dealing in detail with a low habit—in fact the higher overcomes the lower. I think these are the chief points to which I desired to refer.

Mr. J. E. JACK.—It always appears to me to be a contradiction of terms to speak of habits formed within us. The tendencies are inside, but they must be indulged in, and given in to, to grow into habits. We have certain tendencies—we indulge those tendencies and give into them, and at last they *have us*, and they become habits. As to the point whether bad habits can be overcome, to which Canon Girdlestone referred, I have one instance in my mind which exactly meets the case just mentioned. Many years ago I knew a man who was, perhaps, the greatest slave to drink that it was possible to find; he had arrived at that state that he could not lift his hand to his mouth, and he would get the barmaid, or anybody else, to pour the brandy down his throat. He was acted on by moral influence, and he made his wife shut him in his room, and went through, I suppose, the most awful mental process that he could go through, and he overcame the propensity. I knew him sixteen years ago and he is now reformed, and there is not a more sober and upright man in London at the present day: so that it *is* possible to overcome a bad habit such as that. It seems to me that the esteemed author has rather overlooked the moral force necessary to overcome habit. Speaking of training

children in accuracy of speech and not to exaggerate in speaking, which is one of our great vices, a child may become very accurate in his language, but be very selfish, and when a temptation comes to him, to serve his own interests and not to speak with accuracy; will the mere force of accuracy in his training prevent him serving his own interests? A well trained conscience, it seems to me, is the only power that will enable him to overcome tendencies to wrong.

THE PRESIDENT.—If there is no one else who wishes to speak, I will ask the author to reply.

THE AUTHOR.—I have to thank my friends for the very lenient way in which they have dealt with my imperfect paper, and I will just run through one or two remarks that have been made.

I am very pleased to hear the letter from Dr. Hill, corroborating, as it does, so strongly, the view I have taken in attempting to distinguish between moral or mental habit, and habit when it becomes physiological, and I think, perhaps, it is founded on the fact that we are masters of habit up to a certain point; and after that, they become our master; but even then they can be overcome by a higher power and exercise of will. As to what has been said about children, that is very important.

I assent to Canon Girdlestone's substitution of the word *deliberate* for *voluntary* in most cases though, probably, it would not fit all. With regard to the perpetuation of the Chinese women's feet, we need not go to China, unfortunately. We can refer to the English waist as another instance. Although it has been systematically compressed for long centuries, it nevertheless remains at its normal circumference of from 24 to 25 inches; and I think artificial deformity is never perpetuated, whether it be of the feet or waist, as may be the case with a man who has been born, say, with three fingers.

I should like to say one word about moral force in connection with truthfulness, in which I yield the palm to no one; I fully recognise its importance, but it is not the subject of my paper. My idea was rather to throw it out for consideration. Every one knows that truth attains its highest value from a moral stand-point, and I simply alluded to it as giving a power where habit would fail; but the other side of the question, that it can be formed by habit, is, I think, perfectly unknown—at least it dawned on me some years ago as a comparatively new truth. We



all know that habit can be formed, and I think that perhaps I rather omitted to give full force to those admirable remarks which my friends here have given for me. I have only to return my thanks for the way in which you have received this paper (applause).

The meeting was then adjourned.

#### REMARKS ON THE FOREGOING PAPER.

Dr. BIDDLE, M.R.C.S.E., writes:—

I feel convinced that all who read Dr. Schofield's paper, will agree that it gives a most lucid account of the formation of habit. It is only with a few of his incidental remarks that I crave the indulgence to differ. I cordially agree with his denunciation of the late Professor Clifford's *dictum*, "To say, will, influences matter is neither true nor untrue, but simply nonsense." For Clifford, though an excellent mathematician, was an indifferent metaphysician, and a very sorry theologian. But when, in controverting this *dictum*, Dr. Schofield announces that "we have the power of choice, selection, memory, and attention," and that these (all of them) "have no correspondence with any form of nerve action," I must record my inability to follow him. Surely, if he has not heard of that Hebrew scholar who had all the Hebrew knocked out of him by a blow on the head, but whose life was spared, and who re-acquired a knowledge of the language, he must be aware that *memory*, at least, shows itself as belonging in great part to the body, by decaying with it, and otherwise varying in power with the health of the body. I think Dr. Schofield will agree that *habit* itself is of the body and not of the spirit, and is more due to a negative than a positive influence of the latter. It is of great importance to distinguish the powers of the human spirit as being mainly those of *feeling* and *willing*; and these can only be exercised through a properly organised instrumentality. Science is here supported by the Christian doctrine of the *resurrection*, in contra-distinction to the platonic and heathen dogma of the immortality of the soul.

Voluntary actions originate in *motives*; that is, the human spirit wills from some cause which is presented to it.

Heredity and environment go a very long way in the formation of habits, and therefore of character (apart from supernatural

influence); and I am much pleased to find that Dr. Schofield does not agree with Weissmann as to the transmission or not of acquired characteristics. For once Mr. Herbert Spencer has had the best of it.

When Dr. Schofield says that "in what we call voluntary actions all we do is to will a result," he admits that our will-power is very limited; and even in regard to our thoughts this is very evident, as those like Professor Tyndall (who suffered from utter insomnia for twenty-eight days) can tell, when they try in vain to think of *nothing* and go to sleep. In conclusion, I think that most people will agree with Dr. Schofield that facility and grace belong to the automatic, and that actions which are purely voluntary (if there be any such) are, from infancy onwards, always more or less awkward. It is from trying to convert what should be automatic actions into voluntary ones, that much trouble arises to self-conscious people. But there is reason to believe that if the memory of past failures could be eradicated, such neuroses (well called *phobias*) could easily be cured. As it is, the memory itself enters into the circuit, and forms part of the habit, scaring the poor sufferer nearly as needlessly as the cobra did Darwin.

Professor DUNS, D.D., F.R.S.E., writes:—

Dr. Schofield in his interesting paper succeeds in making a difficult subject plain.

Surgeon-General C. A. GORDON, C.B., M.D., writes:—

The paper just read presents a carefully prepared epitome of theories current for the time being in relation to the various points touched upon therein. Taking a few in their order I notice that the subject of localised functions of the brain, whether in respect to intellectual manifestations, or motor actions, although to a certain degree as represented in that interesting communication, is nevertheless modified by conditions the ultimate nature of which has defied detection through the physical means of research heretofore employed, such as are indicated in the paper. In respect to others, conclusions arrived at by investigators differ among themselves to an extent which justifies hesitation in accepting them in their entirety.

In studying the relation of the intellectual faculties to cerebral development, various considerations must be taken into account; among them individual circumstances and surroundings, heredity, national and racial differences, for the circumstance has to be borne in mind that such differences do exist in a degree as marked as are the distinctive physical characteristics which divide them ethnically. The relation in which the aspect of the general subject as here presented stands to others connected with pauperism and criminality among populations opens up a question so extensive in its bearings that it can only be thus alluded to on this occasion.

Adverting to the subject of what have been called motor areas of the brain, I remark further that phenomena observable in disease are in many respects at variance with deductions arrived at by artificial methods of investigation. Other circumstances point to the necessity for enlarging, if not for otherwise modifying that theory. For example, at page 137 of the excellent paper before us, allusion occurs to the "mapping out" of such areas, in the apparent sense that particular muscular movements are directly connected with and dependent upon the regions so indicated; at page 143 the circumstance is mentioned that "pigeons can fly after the removal of the cortex of the brain; frogs similarly treated can balance themselves on a board slowly turned round, and will croak." The whole class of movements to which the name of "reflex" is given tend to indicate their own dependence upon cerebral motor areas.

Facts such as the following are not to be ignored in connection with those remarks, namely, the amphioxus among fishes, and all the invertebrate animals are destitute of a true brain, and yet all necessary functions take place in them. Cases of acephalous children are recorded in which the functions of respiration were performed, and various movements of the limbs took place. Cases are even on record in which though during life no characteristic symptoms were present, post mortem examination revealed extensive cerebral disease.

At page 146 such subjects are discussed as "natural instincts," "artificial reflexes," "physical memory," "psychical habit," &c. In respect to all of which, unfortunately for myself, I am unable to appreciate the precise measure of significance intended to be conveyed by them respectively. No such difficulty occurs to me in regard to the statement at page 147 having reference

to the value of *habit* in the training and efficiency of a soldier; the quality in question besides affecting his physical prowess as a fighting machine, extending also to his *morale*, including what is commonly called his soldierly bearing, as also that particular mental condition in which he has come to accept as a part of his own existence, the preparedness at any time, and as a matter of course, to face the chances of war and battle. Although personally associated with soldiers during all the years of my active life I never became acquainted with such an incident as that quoted at page 151 in respect to "what an automaton a soldier becomes."

Fully in accord with what occurs at page 152 relative to the formation of habits of attention, perfect execution of work and industry, I am convinced that he who to these rich possessions has also the inestimable advantage of having been morally trained as indicated in the quotation from Holy Writ given on the same page, starts on the active business of life, well armed and protected against enemies whose absolute conquest is a necessity for ultimate success, to say nothing of that higher aspect of existence towards which we look forward in hope.

PROFESSOR HENRY WEBSTER PARKER, Ph.D., of New York, writes:—

The paper by Dr. Schofield is one of great interest in subject and is very ably treated. I have marked a number of passages as especially felicitous in statement and to be treasured for future use as quotations. The moral of the subject might, however, have been carried somewhat further. It has been well remarked that the entire system of things, as it relates to good and evil, was designed to work out good, and only by perversion works evil. The evil working goes to illustrate how glorious would have been the right working—even lightens and thunders forth the blessed design. The locomotive engine in its ruinous crash and wreck proclaims that power in the mechanism which, under proper conditions, is grand in its beneficent service. The tremendous force of habit, ill directed, that seems finally to plunge a man helplessly downward, is the very principle that at last renders holy living free, joyous, effortless—a second nature; in a well-ordered life, it lifts and propels ever upward.

On pp. 145–146 I have noted several queries, but pass by as specially figurative the suggested question about physical and

chemical laws originating in habit; also "the habit of all ova to build organisms in accordance with certain exact laws." To me, the meaning is hardly clear. I would only protest that instincts (like those mentioned of the jelly-fish, amoeba, and the ant and bee), spoken of doubtfully as "at first voluntarily acquired habits," and that may "as Romanes suggests, speak to us of lapsed intelligence," offer no evidence of so originating, and therefore no ground for the question Dr. Schofield raises, but considers "unanswerable in our present state of knowledge." It seems to me that the fatal lack of proof of any such origin, and the impossibility of it in the light of both mental and biological science, is just what our present knowledge does give. Dr. Romanes himself presents wonderful instances of seeming intelligence in protozoa, jelly-fish and star-fish, but rules them all out for the reason that in such low animals it is unreasonable to suppose intelligence, although he admits that if we depend on appearances (or analogy, as he argues in his preface) we should have to attribute conscious determination to even microscopical organisms. I have criticised his arguments at length in my work entitled *The Spirit of Beauty*, a copy of which is in the library of the Institute.

Instinct has been the *pièce de résistance* of much discussion. Far preferable to any wild notion of instincts as originating in reason and will, is Darwin's view that they began in chance acts favourable to the perpetuation of species—though of course few can believe that there is any such thing as pure chance. Understood with some qualification, his explanation may be admitted under the category of second causes, while the astounding marvels of complex instinct may still enforce the doctrine of a Divine direction. True, mind (a very general term) may be predicated of all animal life in one sense or another; and we may also favour the view of Agassiz and others that a spiritual element is the organising cause in every embryo-cell, determining its development. But intelligence in animals should be qualified as animal intelligence, or else left to the popular language that ascribes the signal-associated acts of a trick-pony to "extraordinary intelligence." Unqualified, the word is rightly defined as—the faculty of understanding—capability of comprehending facts or ideas.

The question does not turn upon definition and philosophy only. The key of it is in the simplest experiments, which anyone can perform. On the first feeding of meat to a kitten and a puppy,

accompanied by a peculiar call, the association with the call was at once and permanently established. With fish the process was slower but sure and lasting. Whether and what impressions are transmissible to offspring is a wide question. In the *Revue Scientifique*, May 4th, 1889, an account is given of the "formation d'un instinct," to the effect that every evening for ten years (beyond which the narrator's observation did not go) a flock of geese manifested wild terror at a place and twilight hour coincident with a murderous attack that had once been made on them by dogs, although all the older members of the flock had been killed off every year for market.

Aside from the subject of transmission, implied in instinct, one may affirm that all animals below man are perfect mechanisms for the instant or speedy and permanent fixation of every associated sensation and impulse. The first act thus ingrained may be regarded as accidental or as foreordained, according as one may be disposed to interpret the universe. Intelligence, higher in its proper human sphere, is lower than this principle on the plane of animal life, and is worse than superfluous in this matter. The perfection and ruling principle of developed man is intelligence proper. The perfection and operating principle of the animal is quite another thing,—simple and comprehensible as daily illustrated and also as familiar, subordinate and imperfect in our own experience, but so different from our usual, *conscious*, mental action that it will probably continue to be mysterious or misleading to most persons in all time to come.

On this side of the Atlantic, the chimney-swift (*Chactura pelagica*), with the same habit as the chimney-swallow of Europe, is the triumphant instance put forward of intelligent formation or change of instinct. But, first, it does not appear that the original instinct was other than for any high hollow place of nesting; secondly, it is lack of intelligence to affix the nest perilously on a sooty surface, and exposed to rain if not to heat, as many a wrecked nest and dead young swift in the old chamber fire-places abundantly proved; and, thirdly, notwithstanding this, the chimney generations, not being exposed to enemies in the few remaining hollow trees of disappearing forests, may alone have survived, according to the hypothesis of natural selection pure and simple. Beyond this instance we have little except the very natural change of nesting-place by any animal when disturbed, and the crow's speedy association of danger with a gun, &c., &c., all

solvable on the principle of keen and lasting sense-association—more keen and sure than any glimmer of reason. The animals would have fared much worse with a modicum of intelligence. Half-instinct and half-intelligence would each be inadequate, and, together, would confuse and nullify each other. Painful efforts have been made to enumerate and multiply human instincts, but man is not a creature of instinct any more than a brute is a rational creature. The distinction is as sharp and total to-day as ever, although it is quite true that much of human action is on the animal plane of associated sensations and impulses (as when one, without thought drops work at a customary signal)—these connections, however, never becoming instincts simply because reason disturbs and interferes with the process which in animals is as certain as machinery. And this consideration greatly emphasises the radical difference between man and brute—the one rational, the other not. Animal instinct in its very genesis and nature excludes reason. No over-interpreted or under-interpreted facts and strained argument can change the nature of oil and water or mix them. The effort has been to exalt the brute and sink man, for an evident purpose, and agreeably to a mongrel and inverted philosophy that is sensationist when dealing with man and almost spiritual when treating of the brute.

Then too, there is the germ idea which, as Dr. James Martineau remarks, has become the dominant and misleading conception. It would find in a mole-hill the origin of the Alps. There is indeed a dawn of reason in a child, but it becomes full-orbed, and until it is full-orbed, the child is dependent on others. It does not remain half developed to reappear in another being, fully developed. There is no dawn in the moon that becomes sunrise on the earth, nor a germ in the daisy that becomes full-blown in the rose. Moreover, any one element of reason implies all other elements, and is distinguishable only logically. In Dr. Romanes' curious chart of mental evolution in animals, there are fifty provisional steps or levels. On the 21st we have fish and batrachia, with "association by similarity"; on the 22nd the higher crustacea, with "reason"; on the 24th, hymenoptera, with "communication of ideas"; on the 26th, carnivora, &c., with "understanding of mechanism"; on the 28th, ape and dog, with "indefinite morality." Abstraction begins just below the 27th, generalization at the 29th, and reflection at the 34th. But every one of these is implied in the 21st, if that be a noting and notion of qualities by



comparison, in other words by abstracting these. Dr. Romanes himself, in the *Contemporary Review*, vol. iv, regards the higher cognitive powers as resolvable into abstraction. Reasoning is but the linking of general abstract notions (concepts). If any animal can note and consider whiteness, it has all the powers we have, and should be able to consider rightness, and to express in some way its general notions, that is, have language proper, which is made up of abstractions in the shape of concepts. Where is this in the animal world? If instincts be lapsed intelligence, when and where did the intelligence come in?

Returning to Dr. Schofield's paper, I would add that the love of strong drink seems rather too specific as an ingrained inheritance. Is it not rather a general degeneracy, or example and early access to intoxicants? Further, hereditary habit is made to explain the swinging of the arm forward coincidentally with a like movement of the opposite lower limb in walking. If one will try the experiment, he will at once notice that otherwise the body is inconveniently rotated; this might account for an early and independent formation of the habit in every individual. But all my remarks are incidental to the well-treated subject of the paper, though the main point on which I have dwelt is one of important bearing in other relations.

## NOTE.

The author has seen the foregoing. He offers no further remarks.—Ed.

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